JAMES SWAIM.

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MEDICAL MUSEUM.

CONDUCTED

BY

JOHN REDMAN COXE, M. D.

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Observations on the Yellow Fever. By JOHN STEVENS, *Esq. of Hoboken, New Jersey.*

THE following strictures on yellow fever were written during the summer of 1803, when the city of New York was afflicted with this disease. Having myself nearly fallen a victim to it two years before, my thoughts were occasionally turned towards the investigation of the subject on the recurrence of the disease in 1803. What was then thrown upon paper lay by me till the fall of 1805, when it was put into the hands of the editors of the Medical Repository. From the great press of other more important matter, as well as the great length of the manuscript, it remained in their hands until last spring. Seeing no prospect of its appearance in the Medical Repository, I wrote to the editor of the Medical Museum at Philadelphia, signifying my wish to have it inserted whenever he could find room for it; but, not receiving an answer, I did not then send it forward. However, availing myself of the opportunity, by a friend, of transmitting it, I now make him a tender of it, to dispose of in any way he pleases.

New York, August 25th, 1808.

IT is surely high time that the citizens of New York, Philadelphia, Baltimore, &c. should set about, in earnest, to adopt effectual means of exterminating for ever yellow fever from our shores. No exertions, no sacrifices can be deemed too great. Independently of the loss of so many valuable citizens, independently of the distresses and privations sustained by individuals, the mere pecuniary damage, resulting from loss of time, derangement in business, removals of families, and being subject to quarantine in many foreign ports, is incalculable.

The question respecting foreign and domestic origin still remains undecided. This disease cannot, however, with any degree of candour be said to be indigenous, at least on this side of the Chesapeake. Great pains have, indeed, been taken to prove its appearance and existence in places remote from sea-ports, and where no communication with them could have occurred. We are told that yellow fever has raged as violently in the vicinity of the Genesee lake, as it has done at any time in New York or Philadelphia. That fevers of a very malignant type occasionally prevail, and often prove very fatal, in various parts of our country is an unquestionable fact. But of the identity of any of these with yellow fever we have no satisfactory evidence. Nay, I never have yet heard of a single unequivocal case of yellow fever which could not be traced to some sea-port town where the disease prevailed.

If this disease is indigenous, why is it confined exclusively to sea-port towns? And, in these sea-port towns, why does it originate no where but at the wharves? Bilious fever, remitting and intermitting fevers, dysentery, &c. prevail occasionally in all parts of our country. And, I believe, it rarely hap-

pens, during the sickly season, that these diseases do not make their appearance somewhere. Whereas it is a notorious fact, that for fifty years immediately preceding the year 1793, the yellow fever had not appeared any where in our country, not even in the cities of Philadelphia or New York.

In the year 1665 the plague swept off 67,000 persons in the city of London, yet no one ever dreamt that the plague was indigenous in the island of Great Britain. Were yellow fever really generated amongst us, were it merely the effect of noxious exhalations arising from animal and vegetable substances in a state of putrefaction, would it not occasionally occur in different places, whenever these processes, from accidental or other causes, were brought into action? But it is pretended by some that yellow fever, and even the plague itself, are merely higher grades of our common autumnal fevers. Never, perhaps, since the first settlement of the country by Europeans, have fevers and agues, intermitting, remitting, nervous, and bilious fevers, been so universally prevalent throughout the whole of the spring, summer, and fall months, as during the present season. Yet I now challenge the advocates of domestic origin to produce a single unequivocal case of yellow fever originating out of the cities of New York and Philadelphia. Surely throughout so wide an extent of country, during a season so remarkable for fevers of every kind, it is strange indeed that this higher grade should have manifested itself no where but along the wharves of New York and Philadelphia. I am, however, very far from intending to assert dogmatically that our country and climate are incapable of generating yellow fever. All I insist upon is, that from a fair and candid view of facts, this sort of fever ought not to be pronounced indigenous or endemic. But, on the contrary, as it always originates on ship-board, or in dwelling-houses and stores in the immedi-

ate vicinity of wharves, the probability is that it has, in every instance of its occurrence, been derived from a foreign source.

But we are told that the disease is not contagious, and, therefore, cannot be imported. Reiterated experiments seem to warrant the conclusion, that yellow fever is in no case communicable *immediately* from one person to another, in like manner as small-pox, measles, &c. Great numbers of subjects, dying of this disease, have been dissected with impunity. Nay, even the matter of black vomit has been tasted without any bad consequences.

To bring forward individual cases to prove that this disease is not communicable *immediately* from one person to another is at this time wholly unnecessary. They happen every where. The fact, indeed, seems now to be so well established, that the sickening in the country of fugitives from our cities with this disease no longer excites alarm. Even in the hospitals of New York and Philadelphia very few instances have occurred of physicians or nurses taking the disease. And these few instances (in situations where a minute attention to cleanliness is not always to be expected) may be fairly attributed to effluvia arising from articles of clothing, bedding, &c.

But if the non-contagious nature of the disease is thus explicitly admitted, every idea of importation must also be abandoned. This, it must be confessed, is the strong hold of the defenders of domestic origin. Behind this rampart they remain invulnerable to every assault that can be made against them by contagionists. But if it can be made appear that contagion, in the common acceptation of the word, is not necessarily connected with importation, this defence of domestic

origin becomes no longer tenable. To do this I shall presently make an attempt.

It will not, surely, be pretended that the local situations of our sea-port towns are more peculiarly favourable to the generation of yellow fever than other places. Why, then, are these particular spots only, in so wide a range of country, infested with this disease? I shall be told, in answer to this question, that it is owing to the greater accumulations of filth in our cities than is to be found elsewhere. But the advocates of domestic origin do themselves admit that this answer is by no means satisfactory. Mr. Webster declares, "that it is not true that filth and vegetable putrefactions will, in ordinary years, produce this plague." Indeed this concession, to preserve any degree of consistency, was unavoidable on his part. For why had not similar causes produced similar effects, during the long period of fifty years immediately preceding the year 1793? Will it be pretended that since 1793 a total relaxation has universally prevailed in our sea-port towns, and that an unusual accumulation of filth has been suffered to take place everywhere? Mr. Webster, therefore, is perfectly correct when he tells us that filth and vegetable putrefactions will not produce yellow fever. Hence, to account for the appearance of this extraordinary phenomenon, he resorts to a general theory, which he has laboured hard to establish, viz. that pestilential diseases are everywhere the effect of some extraordinary periodical vicissitudes. Now, however he may have succeeded with respect to influenza, measles, scarlet fever, &c. he certainly fails when he attempts to account for the recurrence of yellow fever in our sea-port towns upon the same principles. Let us take his own words. "The autumnal bilious fever is governed by analagous principles, usually appearing in the form of an intermitting or remitting fever,

which is curable, and not infectious, or very slightly so—but in particular years assuming the malignancy of the plague, like the dysentery and ulcerous sore-throat." But where does this ever happen? Not, surely, in the country we inhabit. Where do we ever see "intermitting and remitting fevers assuming the malignancy of the plague?" During the spring, summer, and present autumn, intermitting and remitting fevers have prevailed very generally through the country, but I have never heard of a single malignant case, which has borne the least resemblance to yellow fever. If, from some "extraordinary vicissitude" of season, intermitting and remitting fevers are made to assume so malignant a type in the cities of New York and Philadelphia, surely some symptoms of this malignancy would manifest themselves in other places also. Mr. Webster would not, in this instance, confine the effects of vicissitudes of seasons to mere points, whilst on other occasions, particularly in the instance of influenza, scarcely a quarter of the globe escapes. Effects are ever proportionate to their causes. A general cause must produce a general effect. But that the appearance of yellow fever amongst us does in no shape depend on the vicissitudes of seasons, is evident from the circumstance of its having prevailed for these ten or twelve years past in some one or other of our sea-port towns every season. During this period too we have had wet summers and dry summers, hot summers and cool summers, but without any manifest effects. In short, it may fairly be concluded that this disease is an exotic and not indigenous, or it would certainly show itself in other places besides sea-port towns, and that too at shorter intervals than half a century.

And notwithstanding all Mr. Webster has advanced to induce us to expect a recurrence of yellow fever in our cities at short intervals of time, yet, if it can be kept out for fifty years,

I see no reason why it may not for five hundred. At what interval of time, I would ask, are we to calculate upon a recurrence of the plague in England? It is now nearly 140 years since the great plague, as it was called, appeared in London. Are the periods of its recurrence analagous to the revolutions of some of the comets? No man in 1793 would have ventured to advance this doctrine of periodical recurrence. But, if after a lapse of fifty years, it was then highly reasonable to conclude it a disease of foreign growth, nothing has since occurred to invalidate this opinion. If it was then highly probable that the disease was imported, its recurrence in every instance since, ought certainly to be imputed to a like cause. Since that time our intercourse with the West India islands has been much more intimate and extensive than it had ever been before; and since that time, too, this disease has prevailed in these islands more generally and with more virulence than was ever before experienced. Since the war we have become the carriers of the produce of the French, Spanish, and Dutch colonies. If it is, as indeed Mr. Webster himself admits it to be, an importable disease, is it, I say, to be wondered at that it should from time to time have been introduced amongst us? Indeed, taking all circumstances into consideration, it would have been next to a miracle if it had not.

Let me now bestow a few words on the nature of this disease. Mr. Webster adopts the opinion of Dr. Rush and other physicians, who conceive yellow fever to be only a higher grade of our usual autumnal intermitting and remitting fevers. He tells us that the autumnal bilious fever usually appears in the form of an intermitting or remitting fever, but in particular years assuming the malignancy of the plague. Hence he denominates yellow fever "the bilious plague." I am no physician, but I will take the liberty to assign a number of reasons

which convince me that yellow fever bears no affinity to intermitting or remitting fevers, or to our usual autumnal bilious fever. These fevers prevail occasionally throughout every part of our country; whereas yellow fever is a disease which never appears any where but in sea-port towns. Intermittents are very common in the spring, disappear about midsummer, and re-appear in the fall: whereas yellow fever never appears before midsummer, at the very season when intermittents usually cease. Nay, there seems to be a repugnancy between the two diseases; for an intermittent is now generally considered to be a protection against yellow fever. Persons of diametrically opposite temperaments are the usual subjects of the two diseases. Yellow fever attacks the middle-aged man of a plethoric sanguine habit; whereas women and children, and those who from poverty of living, or from other causes, have poor blood, are the usual subjects of intermittents. Surely, if these maladies were merely different grades of one and the same disease, a predisposition to the one would prove so also to the other.

It is not true that in yellow fever the bile is the primary seat of the disease, which appears to be universally the case in intermitting and remitting fevers, in short, in all disorders arising from marsh effluvia and vegetable putrefactions.

All malignant fevers are more or less attended with eruptions on the skin, which indeed serve as discriminating characteristics of the disease. Yellow fever has also its peculiar species of petechiæ.

I shall not presume to go into any investigation respecting the constituent principles of that specific gas which causes yellow fever. The subject seems yet to be enveloped in obscu-

rity, concerning which the learned have advanced the most contradictory opinions. Thus far, however, I shall venture to state an opinion, that, whatever it may be, it is not the product merely of animal and vegetable putrefactions, because, as I have more than once urged before, the disease would not, in that case, be confined merely to sea-port towns, but would manifest itself generally through the country, wherever processes of this nature occurred.

The cause or causes of yellow fever, whatever they may be, are exclusively confined in their operations to the places where it prevails. During the present season there has not been the most remote analogy subsisting between this disease prevailing in the city of New York, and the diseases prevailing in the country round it. Does not this remarkable circumstance lead us irresistibly to this natural conclusion, that yellow fever as well as plague, beyond the tropicks, are everywhere to be considered as exotics; but, like hot-house plants, though they may be made, under favourable circumstances, to strike root and even to flourish in colder regions, yet, when exposed to the native rigour of the climate, sicken and die?

The introduction and prevalence of the plague were infinitely more frequent formerly throughout the southern parts of Europe than at this day. This exemption at present is unquestionably owing principally to the superior vigilance and rigour with which quarantine laws are now enforced in these countries; perhaps it may be owing in part to improvements in police regulations, and in the mode of living. Colonel Wilson informs us, that "the plague has long been supposed to have been brought from Turkey in the ships charged with old clothes, which constantly came to Alexandria for a market. But the plague has generated annually in Egypt during the

last four years (although no such communication has been possible), and even chiefly commenced in Upper Egypt." "In Cairo, last year, forty thousand people were supposed to be infected with the plague, and many of the French garrison died in that city, although the disease was treated in their hospitals with the greatest ability. In Upper Egypt, sixty thousand of the inhabitants perished during the same season. There whole villages were swept away, and remained abandoned when the Indian army descended the Nile."

Here then are we furnished with the most incontestible evidence of the plague being an indigenous disease in Egypt. It raged everywhere in the *villages* throughout the *interior* parts of the country, so much so that in Upper Egypt sixty thousand perished in one season. Contrast this universality of plague in Egypt with the contracted locality of yellow fever in the United States. Were all the ground now occupied by the towns in the United States, where yellow fever has ever appeared, annihilated, would any perceptible chasm be discovered? Not a hundred thousandth part of the territory of the United States would be lost, yet in such an event would yellow fever be totally extirpated. Can this be true, and the disease with any degree of propriety be called indigenous? Colonel Wilson further says, that during this season the plague did not show itself either at Rosetta or at Alexandria. That these towns should escape in such a season, would indicate a probability that plague in these places generally derived its origin from importation.

(*To be continued.*)

An Essay on the Partial Inversion of the Uterus. *By* William P. Dewees, *M. D.*

PREGNANT and parturient women are peculiarly liable to sudden and dangerous diseases; it would therefore seem a duty incumbent on every practitioner to relate whatever might tend to lessen or prevent their recurrence. The uterus is subject to such variety of derangements, that almost every day affords something new to the accoucheur. Of this kind is the partial inversion of this viscus. From the nature of the functions of this important organ before pregnancy; from the duties imposed on it by conception; from the efforts which it must exert at the ultimate period of gestation, we are obliged to regard it as one of the most interesting viscera of the female system. But unfortunately this very importance subjects it to peculiar diseases, many of which are so sudden and dangerous, that, if not almost instantly remedied, the patient inevitably dies.

As far as I am acquainted, no one has noticed the partial inversion of the uterus: many cases are on record of its complete inversion and protrusion from the vulva, all of which, as far as my recollection serves me, have proved fatal; but no mention is made of death from its being partially inverted, and where this viscus is still confined to the cavity of the pelvis. Four cases of this kind have fallen under my notice within the last eighteen months, for the detail of which I shall make no apology, as their importance, I trust, will completely justify their promulgation, although it may not warrant my speculations on the subject; but these shall have the merit of being short, if they be not interesting.

By partial inversion I mean where the fundus of the uterus has passed either through the os externum, or is turned down inside out as far as the neck of this viscus. This takes place, I am disposed to believe, but at the full or very near the full period of gestation, as before this time the uterus is not sufficiently distended to subject it to this accident.

For the inversion to take place several circumstances must combine: first, the body and neck of the uterus must remain flaccid, and the fundus contract after the expulsion of the child*; and secondly, it may be essential that the placenta be ingrafted on the fundus.

The remote cause of this disease is, whatever may prevent the contraction of the uterus. The uterus may lose its contractile power from over-distention; this may happen from an excess of the liquor amnii, from the unusual size of the fœtus, or from compound pregnancy; it may lose this power from hæmorrhagy, either before or immediately after delivery; from passions of the mind; from exhaustion by previous disease; from external violence; from its long-continued efforts to effect delivery, &c.

The proximate cause is, whatever may be capable of drawing the fundus down while the remote cause exists: this may be the placenta ingrafted at this part, or perhaps, in some cases, the mere weight of the fundus itself.

* My reasons for this opinion are: first, when the disease exists, we find the tumour very firm, while the body and mouth may remain flaccid some time; and, secondly, because we find the placenta detached, which probably would not happen had not the fundus contracted so as to throw it off.

The indications are simple: the reduction of the fundus, when it has not passed too far through the mouth of the uterus; and, when passed too far for restoration, to take off the stricture occasioned by the mouth through which it has passed contracting too forcibly on the body*, and thus producing disturbances and consequences similar to those which arise from a portion of gut being strangulated.

The first indication is to be fulfilled by placing the back of the fingers against the tumour after the placenta is removed, and pushing it in the direction of the axis of the uterus until the fundus is restored to its natural situation†. Should we find the body of the uterus too flaccid to retain the fundus in its proper place, we ought gently to stimulate it by rubbing the fingers against it until it contracts sufficiently; nor ought the hand to be withdrawn until this effect is produced. I do not know from experience that this extreme torpor may exist in the case we are speaking of, but think it possible; we shall have therefore two difficulties to contend with, in this situation of the uterus, namely, a disposition in the fundus to prolapse, and, secondly, hæmorrhagy, both of which will perhaps be better obviated by the irritation produced by the presence of the hand than by any other means. But, in case of alarming hæmorrhagy, I would not solely depend on the presence of the hand to produce contraction, but would give the acetate of lead freely, and have a stream of cold water poured on the abdomen. We have nothing to apprehend from this procedure,

* See cases III and IV.

† By fundus is to be understood all that portion of the uterus above the insertion of the Fallopian tubes; by body, all that portion between them and the neck.

as it is warranted by experience in hæmorrhagy, not attended with a prolapsus of the fundus.

As soon as the fundus is completely pushed up, and we perceive the uterus to contract, we may safely withdraw the hand.

The second indication is to be fulfilled by grasping the tumour firmly, and drawing it towards the os externum pretty forcibly; by this means we make the body of the uterus pass through its mouth, which is the contracting part. This, I believe, will always be easily effected, as the prolapsed part passes from a greater to a lesser bulk, in proportion as we approach the mouth; for the uterus, as soon as emptied, will return more or less to its pear-like shape. It may be proper to observe, if this case be of any standing, and the bladder not empty, the urine should be drawn off by the catheter. See case II.

This disease may be suspected when the following circumstances obtain: first, where we find the placenta very bulky and firm at the os externum soon after the expulsion of the child, and that it gives more than ordinary resistance to delivery when it is attempted by an exertion at the cord: secondly, when we have applied as much force as the cord ought to bear when the placenta is thus low in the vagina, and we do not find it advance; if the patient complain of much pain from this exertion; and, more especially, when we attempt to aid the force applied at the cord, by hooking the placenta with a finger, and still find it give uncommon resistance: thirdly, when the patient complains of much pain, has some hæmorrhage, is very faint, has cold sweats, and becomes extremely pale, more especially when this paleness cannot be accounted

for from the quantity of discharge*. Under these circumstances, we ought to desist from all attempts to deliver the placenta, until we examine whether the fundus be not prolapsed with this mass. For this purpose, we should either pierce the placenta with the fore finger of the left hand, and tighten the cord with the right, or should search for an edge of the placenta, and trace this to the place of adhesion; if we find there a round, solid, and rather rough surface, we may be sure that the difficulty to the delivery of the placenta arises from a prolapsus of the inverted fundus. Having ascertained the nature of the difficulties to be overcome, we introduce the left hand, if the patient be on her right side, and the right should she be on her left side, or either if on her back, and carefully separate the placenta by insinuating the fingers between it and the uterus: after it is separated it may be withdrawn by the hand that is without; we then proceed, as has been already directed, to restore the fundus.

We are generally cautioned against any violent attempts to deliver the placenta: this advice I conceive founded on just principles, but not precisely on the one for which it seems especially recommended. The only object of this admonition, with the generality of those who have urged it, is to prevent an inversion of the uterus; but this fear is founded much more in theory than in practice, in the generality of cases, as it would be necessary to this accident, that the placenta should be ingrafted on the fundus, and the uterus to be in a state of atony. But experience constantly proves that the placenta is not more (nay, I may very certainly say not so frequently) attached to the fundus as to other portions of the uterus; and if it be not attached to this part, any force applied to the funis could not

* See case IV.

produce the inversion, since it could not act in the direction of the axis of the uterus, through which the fundus must fall. Indeed, I believe it may take place even after the expulsion of this mass, from the simple weight of the fundus itself. But I do not believe this accident can happen when the placenta is attached to the body of the uterus, at least while it preserves that attachment, as this mass will serve to keep up the fundus. Now, as I never would have the rule dispensed with, as the circumstances favourable to an inversion may combine, and would only require a small force in addition to the weight of the placenta to produce it, I would urge that we should not exert any force on the cord until we have ascertained, by externally examining the abdomen, that the uterus had contracted. I by no means believe a force on the cord to be absolutely necessary to produce this terrible disease, yet I can very readily conceive, that in a given case (where the atony was either not very great, or only partial) this force, other things being equal, might bring on an inversion, from which, had it not been applied, the woman might have escaped. The cases I am about to relate are, I conceive, proofs of the first of these positions; for it appeared, on strict inquiry, that no force had been applied by the midwives in cases I, II, and IV, and, as case III was entirely under my own management, I am enabled confidently to assert, that force is not necessary to this species of prolapsus. I am the more anxious to insist on this point, that practitioners, as well male as female, may not be indiscriminately blamed when this untoward case may happen. I would farther urge, that where more than ordinary force appeared necessary to deliver the placenta, or, in other words, if the placenta, when plainly felt in the vagina, cannot be delivered by the force commonly proper to be employed, that we should not persist in this force, until we had satisfactorily ascertained that the fundus was not prolapsed with the placenta.

From four cases of this disease having happened in such quick succession, I am disposed to believe this accident to have happened more frequently than we might at first sight be willing to allow, as no mention (as far as I know) is made of it by the writers on this subject. May we not reasonably trace many of the fatal hæmorrhagies after delivery to this source? may we not attribute other instances of death to this cause, where the disease was neither sought for nor suspected? do not some of the fatal syncopes which sometimes take place after delivery depend on this state of the uterus?

CASE I.

On the 2d of July, 1807, at ten o'clock, A. M. I was called to the wife of Samuel N——, in labour with her first child. Her pains were weak and irregular, but pretty frequent; presentation perfectly natural. As every thing appeared promising, I left her to the care of her midwife. At four o'clock, P. M. she was suddenly delivered; considerable hæmorrhage with faintings followed. I was again sent for, but did not see her until six o'clock, as she lived at some distance from the city. I found her without pulse, cold, and covered with perspiration; with laborious and hurried breathing; the placenta not delivered, and the hæmorrhage continuing. I ordered her such remedies as appeared most pressingly indicated, and immediately examined her per vaginam. I found the placenta just within reach of the finger, and attempted to withdraw it, but it gave great resistance and extreme pain. I now introduced my hand, and found a tumour resembling in shape and size the indentation at the bottom of the common black bottle, over which the placenta was spread. This case was perfectly new to me, although I strongly suspected the nature of the disease. I searched for the detached portion of the placenta,

from whence the flooding proceeded, and carefully detached this mass from the tumour; I then endeavoured to push up this body, but quickly desisted, from the extreme pain it occasioned, and the uncertainty that it was the proper mode of proceeding to procure relief. My patient died in half an hour.

I obtained leave to inspect the body, and Dr. Rush very kindly accompanied me. It proved, as I had previously suspected, to be a partial inversion of the uterus. I dissected out the uterus, which was still so flaccid as to be turned inside out with as much facility as a soaked bladder. The fundus dipped into the body of the uterus about three inches.

REMARKS.

The extreme situation in which I found this patient, renders it very doubtful whether the reduction of the uterus would have been attended with any advantage, but had I had the knowledge of the disease that I now have, I certainly should have attempted it. It may appear to some who speculate on these subjects in their closets, that I failed in enterprize; but let it be recollected the disease was perfectly new to me; that the poor woman was absolutely in articulo mortis; that the pain of the attempt was extreme; and at the moment I believed, that even reduction, were it a prolapsed fundus, would be unavailing, will prove an apology for not persisting, and prevent the charge of suffering a patient to expire before my eyes, when there was a chance of relief. The death of this poor creature was more owing to the immense loss of blood, than to the prolapsus; and the hæmorrhage must be considered as proceeding from the uncontracted state of the uterus. It may be asked how this could happen: the uterus be in a state of relaxation, sufficient to give

rise to a fatal hæmorrhagy, yet offer so much resistance to the reduction of its fundus? The answer is at hand. It is well known that the different parts of the uterus may be at one and the same time in opposite conditions; that is, one portion may be in a state of contraction, while another may be in a state of relaxation. See Baudelocque, p. 146, vol. I. Thus, then, I conceive this case to have been; the fundus of the uterus is never, I believe, sufficiently ample to receive the whole of the placenta, consequently portions of this mass will be attached to its body; this part, from some cause not sufficiently obvious to mention, was at the moment of delivery in a state of atony (or, as Baudelocque emphatically calls it, syncope); the weight of the placenta dragged the fundus through the flaccid walls of the body, while the fundus retained its power of contraction; this contraction would separate a portion of the edge of the placenta from the body, and thus expose the vessels that were before shut by its attachment; some of these vessels are large, and will in the course of a short time pour out an immense and deadly quantity of blood. The uterus will recover its contractile power sometimes even in the moment of death. This I believe to have happened in this case, as the fundus itself was very firm, and the body, as I have already observed, gave a resistance not to be overcome by the force I used. I did not employ much power, but more than sufficient, I am certain now (from experience), to have carried the fundus through the body had it been still flaccid. In case III, I succeeded in the reduction of the fundus with a force not greater than that employed in case I.

The uterus may regain or retain its power of contracting in the moment of death*; it may also lose it again at this mo-

* See Harvey, Baudelocque, &c.

ment. This took place in this case, for at the time of opening the body it was in every part perfectly flaccid. Does this not prove that its action does not depend on elasticity, as has been asserted by some? does it not prove its muscularity?

CASE II.

On Friday, 24th March, 1808, at half past 5 o'clock in the morning, Mrs. P. was delivered of a living child; her waters discharged themselves six or seven hours previously, and before her midwife was called. The placenta came away spontaneously, as the midwife asserted, and to which the patient herself agreed; its expulsion was attended with great pain, and great flooding; she vomited severely for an hour, and several times fainted without an abatement of the discharge. This, however, was eventually moderated by the acetate of lead, and perhaps contraction of the uterus itself.

After this she continued pretty tranquil, but weak, until Sunday morning, when there was a renewal of the hæmorrhagy, with pains resembling those of labour. These ceased in the afternoon; but she became more alarmingly ill. She now fainted frequently, and the discharge continued. In this way she kept until Tuesday, at which time I was called, at the desire of Dr. Atlee, whose patient she now was. The doctor suspected the true state of this woman's case, and mentioned his opinion to me, to which at first I could not assent, as all the cases I had ever heard or read of, as well as I recollected, had proved fatal almost instantly; and the case I had witnessed but a few months before but served to make me doubt the doctor's representation, or rather opinion. Here, were his judgment correct, was an instance of inverted uterus of four days' standing: a case giving contradiction to all I had heard

or believed on the subject. I however visited the patient by appointment, and found her almost exhausted; her pulse so frequent as not to be numbered, and so small as scarcely to be perceived; great difficulty of breathing, and became faint on the least motion; insatiable thirst, frequent vomiting, cold extremities, and a continuance of uterine discharge. I examined her, and found, as Dr. Atlee had declared, the uterus to be inverted. The fundus was down at the os externum, and could readily be seen partially covered with a thin coagulum of blood when the labia were separated. The places not hid by this coagulum were rough or spongy, and of a dark brown colour.

A very dreary prospect presented itself by ascertaining this poor woman's situation; we believed death to be inevitable. But one resource offered itself, namely, to attempt the reduction of the fundus, hoping, as the uterus had not escaped from the vagina, the inversion might not be so complete as to render this impossible. We accordingly proposed this attempt to the husband and friends of our patient, candidly stating her situation, and the almost certain result if relief was not obtained in this way. They without hesitation submitted the case to our management.

We carefully drew her to the side of the bed, and had the knees drawn up and supported. I gently introduced my hand under the tumour, and gradually raised it; this gave me sufficient room to examine the nature and extent of the inversion. The instant I raised the womb there was a large and sudden discharge of urine: this gave still more freedom to an examination that was to terminate in the disappointment of my hope of the reduction of the fundus. I found so much of it had passed through the mouth of the uterus as to render any attempt at reduction futile, and the more especially as the tumour

was augmented by its having swelled since its prolapsus. The stricture occasioned by the contracted mouth was readily felt, and was very strict. I was extremely perplexed for the moment how to proceed, or to announce the failure of an attempt that alone at first sight appeared to promise success or even relief, but it fortunately occurred to me, before I withdrew my hand, that I might take off the stricture by inverting the uterus completely. Agreeably to this suggestion, I grasped the tumour firmly, and drew it pretty forcibly towards me, and thus happily succeeded in slipping the remaining portion through the constricting mouth. The woman was almost instantly relieved from much of the anxiety and faintiness she had before experienced; but as she was so exhausted by previous suffering and discharges, and as the internal surface of the uterus was now exposed to the influence of the external air, I was prevented from feeling or giving the slightest encouragement of recovery to her friends; but fortunately the event proved how groundless were my fears, for from this day she rapidly recovered, without another alarming or troublesome symptom.

Milk was freely secreted on the fourth day after, and continued freely. Our patient was twenty-three years of age, delicate, but always healthy, but more especially so during her pregnancy.

I visited this patient to-day, November 26, 1808, and found her at the wash-tub, perfectly well; suffers no inconvenience whatever from the uterus; menstruated regularly for three periods; had more or less discharge of mucus tinged with blood for four months; this last four months has had no discharge of any kind; suckles her child, which is remarkably thriving. The uterus is so much contracted as to be no longer within reach of her finger.

REMARKS.

In this case we see with what wonderful facility parts accommodate themselves to new situations; the mouth of the uterus is now within the abdomen, while the once internal surface of this viscus is subjected to the action of the external air, but whose influence it appeared to resist for some time, as it persisted for three months in the regular secretion of the menstrual blood. Nay, we do not know whether this is stopped even now by any change effected on its now external surface; it may be the natural interruption from suckling. May this woman again conceive? I do not believe it impossible. It is a case well worth watching, for should this woman again prove pregnant, it will effectually settle a long disputed point of physiology; it will incontrovertibly prove that the semen is not conveyed through the os tincæ to the cavity of the uterus, from thence to the Fallopian tubes, and from thence to the ovaria, to produce conception.

CASE III.

On the 23d of November, 1808, Mrs. G—— was suddenly delivered of a large female child, which breathed and cried freely immediately after its birth. The funis was not cut until after the pulsation in the cord had entirely ceased, which was in about ten minutes. After the child was taken away, I took hold of the cord, and merely tightened it, on which she begged me to wait, as it gave great pain. I, however, traced the cord to the vagina, and found at the os externum a placenta I thought unusually dense and large. On gently attempting to withdraw it, as I thought it loose in the vagina, I found uncommon resistance, which I attributed to its bulk, and desisted

from farther effort, hoping the uterus would by contracting push it completely down. In this I was disappointed;—some hæmorrhage ensued. I now expected a more than common cause detained the placenta in the vagina, and began a more minute examination. I pierced the substance of the placenta with the fore finger of my left hand, and tightened the cord with my right; beneath the placenta I perceived a round hard substance, which I but too quickly discovered to be the fundus of the uterus inverted. I immediately introduced my hand into the vagina, and found the detached edge of the placenta from which the discharge proceeded. I carefully separated the whole of this mass, and withdrew it from the pelvis without the least difficulty. A considerable flooding ensued.

As Mrs. N——'s case (case I) gave me a complete insight of the mechanism of this displacement of the fundus of the uterus, and as I had resolved to attempt its reduction if ever an opportunity again offered, I instantly, after withdrawing the placenta, introduced my hand, and pressed the prolapsed fundus firmly with the back of my fingers, and carried it upwards in the direction of the axis of the uterus, and in less than half a minute succeeded completely in restoring it. Mrs. G—— has not had a single unpleasant symptom.

REMARKS.

The success attending this case warrants, I conceive, the hope that this formidable disease may always be relieved, if means be promptly used. It points out the necessity of a careful search in the vagina, where unusual difficulty attends the expulsion of the placenta; where there is hæmorrhagy, and the placenta found at or near the os externum; but, above all,

when great pain is felt when any force is exerted on the umbilical cord.

CASE IV.

Mrs. G—— was delivered on the 25th December, 1808, at six o'clock, P. M. after a labour of some hours, of her first child. The placenta was extracted in about fifteen minutes without force. There was some hæmorrhage, and considerable pain. She was put to bed, and became very faint, and complained of great pain, which was occasionally augmented. She continued in this way, only gradually becoming worse, until nine o'clock, at which time I was sent for.

I found her with a small frequent pulse, great anxiety, extremely pale and cadaverous, and in a profuse cold sweat. I inquired respecting the flooding; but this did not appear to be sufficient to account for her present situation. I immediately suspected a partial inversion of the uterus, and thought proper to apprize her friends of the probable cause of her distress and danger, and of the possible result of it. Every thing was left to my management. Upon applying my hand to the abdomen, I found the uterus sunk pretty low in the pelvis, and indented at its top. I immediately after examined per vaginam, and found my conjecture but too true.

The uterus was found inverted, and its fundus was just within the os externum. I was much alarmed for my patient, as three hours or rather more had elapsed between the time of her delivery and my being called; she was much exhausted, and in extreme agony. I quickly introduced my left hand into the vagina, and applied the back of my fingers firmly against the tumour, while I moderated its influence in carrying the

uterus directly up through the pelvis, by having a gentle pressure made upon the abdomen above it. The tumour soon began to yield, and in about two minutes the fundus was completely restored.

On the third day after, my patient complained of a severe pain in the right side just above the ilium, for which I bled her freely and purged her briskly. Nothing unpleasant supervened after this; she might be said to have had a good getting up.

REMARKS.

Three hours was lost in this case, from a belief that all the pain and anxiety was owing to after pains as they are termed; but when 120 drops of laudanum did not relieve her, the midwife became alarmed, and I was sent for. This patient would have been spared much distress had the disease been instantly known; and the risk of death prevented, had the uterus been quickly replaced. I say risk of death, for this there certainly was, as her symptoms were as alarming as possible; nor was there any ground from experience to hope for a reduction of the fundus, as so much time had been lost. This case I deem highly important, as it teaches us not to abandon our patient under these circumstances, and to attempt reduction at whatever time we may be called. We certainly cannot limit the time at which this attempt shall be rendered unsuccessful; this may in some instances happen before the period of three hours, or perhaps one; and it may be possible at even a later period. May not the disposition to syncope in this case, have retarded the contraction of the body and neck?

It was insisted on, in this case, that no unusual force was used to deliver the placenta; it separated from the uterus spon-

taneously, and was expelled without introducing a finger into the vagina.

Philadelphia, 30*th January*,
1809.

Observations on Cold. By "RADCLIFFE."

SIR,

IF the accompanying attempt to burlesque what I conceive to be the sophistical arguments respecting the stimulant nature of cold, should not partake of too much levity and trifling to comport with the plan of your Museum, you are welcome to insert it. I am aware that it can hardly be admissible, even when viewed as I intend it; but my opinion of the subject is such, from the arguments I have heard, that I believe something of this nature would place it in a fairer view than serious reasoning. I know some of our most respectable physiologists think differently, and I would wish to treat no one with disrespect; but we all know that some of the most absurd theories have been entertained by the greatest men.

I am, sir, with respect, &c.

Dr. J. R. Coxe.

"*Difficile est, non satiram scribere!*"

DR. COXE,

In consequence of the arguments I have heard advanced by some ingenious speculative gentlemen of the present day, and especially from a perusal of the communication of "*Arbuthnot*," in the fifth volume of your Museum, I have been induced to pay some attention to the theory which they have advo-

cated respecting the nature of *cold.* They contend that cold is a stimulating agent; and some with whom I have conversed, acknowledge a belief in the existence of *frigorific particles.* That such is the opinion of "Arbuthnot," I take for granted; because he will hardly deny that heat is a stimulus; and if he admit that cold is a mere abstraction of heat, it would involve a paradox unworthy of that gentleman, to suppose we could increase stimulation by subtracting stimulus. It would be a novel kind of arithmetic indeed, to contend that by subtracting we add! He must therefore assume, as a datum, the existence of the *matter of cold;* which matter, being applied to the living system, produces excitement.

Now I have to inform you, that the ingenious arguments and suggestions in favour of this theory have led me analogically to the discovery of other theories; or rather, I have extended this theory to other matters, which, I trust, when stated, will forcibly illustrate the doctrine of the stimulant nature of the matter of cold. As the phenomenon of cold has been explained by supposing an influx of frigorific matter, so I have supposed that *dryness* was caused by the presence of what I propose to call *siccitatiferous particles*, or *matter of drought;* and that *darkness*, in like manner, was the effect of a preponderance of *tenebrific matter.* The analogy between our theories must be very evident; and I hope will appear still more striking, when we have traced them further, and considered their similarity when applied to the phenomena they explain.

"Arbuthnot" has perspicuously stated the numerous cases in which cold acts as a stimulus in common with *other* stimulant matters; and I think the advocates for frigorific particles must see in his arguments what *they* would deem good proof of the existence of those particles. I know, indeed, it has been

said, that cold is proved to be a mere absence of the matter of heat, by the expansion of bodies when heated, and their contraction, or diminution in bulk when cold: but, on the other hand, it may be said we have a proof that the matter of cold exists, in the bursting of an egg-shell (and other containing vessels), when a copious influx of frigorific particles takes place: and again, in Wedgewood's pyrometer, we observe a diminution in size when the frigorific matter is expelled. (For further proofs, vide *Musschenbroek et al.*) So it has been generally supposed that *drought* was only an absence of moisture, as evinced by the swelling of substances impregnated with the latter, and their subsequent contraction upon its expulsion; but we all know that a cord, or rope, will shrink when impregnated with moisture, and expand again when saturated with the matter of drought.

"Arbuthnot" infers that cold is a stimulus, because it excites "sensation" and "motion," which are the acknowledged effects of a stimulating power: so we observe, that the "moving fibres" of a fish are powerfully excited by the presence of siccitatiferous particles; convulsions and death are the result of a complete application of them. That "sensation" is produced in this case, as well as "motion," will hardly be denied; and we have a proverb in corroboration of the opinion, which is applied to persons who are suffering great *uneasiness:* they are likened to "a fish out of water." A dry tongue and fauces also produce uneasiness; in fact, the *positive* effects of a superabundance of the matter of drought are numerous and well known.

The same observations apply to the *tenebrific particles.* A predominance of those particles cause a dilatation of the pupil, and consequently a "motion" of the fibres of the iris, which is

one of the characteristics of a stimulus. That this "motion" is the effect of stimulus, we have the analogical proof of the same result being produced by the effusion in hydrocephalus, and by the exhibition of certain medicines, which every one will admit to be stimulating! As to the "sensation" caused by an influx of tenebrific matter, every person must be conscious of it on going suddenly from a strong light into a chamber which is replete with that matter. In addition to this, we have the authority of one of the greatest poets, a poet who was long enveloped in "tenebrious gloom," and who speaks of "darkness *visible*," of course, producing "sensation!"

"Arbuthnot" says, "a hot or cold bath, or even immersion of the feet into hot or cold water, *equally* and instantly produce shivering." So, in like manner, is ophthalmia *equally* cured by the application of tenebrific matter, and by scarification, and collyria of sulphate of zinc, which are known to be stimulating! "*When two causes produce precisely the same effect, is it not perfectly just to conclude that both act in the same way, and therefore that both are stimulants or both sedatives?*"

Whether I have adhered to the "strict accuracy of logical reasoning" or not, must be left to the logician to decide; but I humbly presume, that the same logic which enables "Arbuthnot" to prove cold a stimulus, will bear me out in the inference that darkness and drought are also stimuli. As to the correctness of Cullen's definition of stimulus, I would beg leave to observe, that, if my theory be not admitted, I shall entertain doubts; because I believe I have shown that darkness will cause both "sensation" and "motion!" and I shall be induced to think, that if my arguments are called sophistical, the same epithet will be equally applicable to those in favour of the stimulant nature of cold. I believe both theories stand on the

same basis, supported by the same kind of arguments; and on the same foundation I leave them, to be supported or exploded, as the good sense of the scientific may determine.

RADCLIFFE.

January 23, 1809.

P. S. There is one point in which the application of cold to the living body may possibly not have been generally viewed, and in which it may probably produce a stimulant effect. We know that when a very cold substance is applied, there will be a rapid transition of caloric from the animal body to the substance in contact with it; and it is very possible that the rapid passage of caloric *out* of the body, may produce an effect similar to that which is caused by an *influx* of the same stimulant fluid. It has been said that frozen mercury produces the same effect on the living body as a hot iron; the only difference being in the opposite direction of the caloric, in the former case passing *out of*, in the latter *into*, the body. If this be a fact, why may we not suppose the same principle to hold good when applied in a less degree? If the efflux of caloric be very gradual, there will be little or no stimulation, it is true; but if it be copiously and rapidly drawn through a part, no matter in what direction, it must stimulate. I merely suggest this idea, not as being a new one, for I have heard it advanced, but because I believe it has not been sufficiently noticed in the discussions respecting the physiological agency of cold. R.

Account of the Influenza, as it prevailed in Waterford, Saratoga county, New York, in 1807. *By Dr.* JOHN STEARNS.

Waterford, Saratoga county, New York,
23*d August*, 1808.

DEAR SIR,

THE winter and spring of 1807 were marked by no epidemic or uncommon disorders. A greater variety of cases of typhus and phthisis pulmonalis was all that distinguished this from preceding years. These cases, however, had entirely subsided some months before the approach of the influenza. The first case of this epidemic appeared in this village on the 7th of August. In Albany, ten miles south, it had prevailed some days previous, and was introduced into this village by those who had been attending the courts then holden in that city. Mr. James, who was first affected, had taken lodgings in a family consisting of fifteen persons; some of these occupied stores in different parts of the village. Excepting those who had taken the influenza in Albany, the members of this family were first affected; and I think I could trace the first symptoms of disease in those who had been most familiar with the sick. About the 13th of August the disorder had become general, and continued to prevail till the 18th, when its decline had evidently commenced, and by the 30th not a single case remained in the village. In the country adjacent, however, it did not disappear until the 6th of September.

I observed that males were generally affected before the females, particularly where their occupations exposed them most to the external atmosphere. Those women who were

exclusively confined to their houses escaped. Children were less affected than adults, and negroes still less than children.

Its progress was nearly from south to north, and appeared to be accelerated by an intercourse with affected places. Although it did not wholly depend on this intercourse, yet it was evidently a mean to facilitate its diffusion.

That this epidemic was communicated by some peculiar change in the atmosphere, is an opinion that I have been hitherto inclined to embrace. But the facts respecting its progress, which I have this season observed, conduce to a belief that it depends on some specific contagion. How shall we otherwise account for its first appearance in the family where Mr. James boarded, and for its gradual diffusion from that source? Why was not its appearance in different parts of the village simultaneous, and not dependant, in some degree, on an intercourse with diseased persons? The place where it first appeared was a public house, and situated at the northern extremity of the village: had it been wafted by the atmosphere, we should have expected its first appearance in the southern part of the village: through this part there is no public road, and this was the last place affected. These facts at least excite a doubt, and, taken in connection with others of a similar import, may be decisive of the question, whether this epidemic is propagated by contagion?

The influenza was ushered in by the following premonitory symptoms: a listlessness and lassitude, succeeded by a pain in the head (particularly about the frontal sinuses), back, and bones, with a general soreness in the muscles. The throat was also affected with a soreness that gradually extended to the trachea and ramifications of the bronchia. The irritation pro-

duced by the inspired air excited a violent cough, which was the last symptom that disappeared. A sensation of general distress was induced, which the patients were unable to describe; the stomach was affected with nausea and oppression. Where sneezing was a common symptom, the disorder was mild, and had a sudden termination: the discharge in these cases was so acrid as to inflame the nose and upper lip. The tonsils in some were tumified and extremely sore; excruciating pains in one or both ears affected some; ophthalmias, in some cases, were severe, and extremely painful. The pulse was generally small and slow, with very little tension. I found but two cases that required bleeding. The remedies were generally emetics, cathartics, sudorifics, and diaphoretics *pro re nata*. The cough was successfully treated with a tincture of the balsam of Tolu.

It terminated in different affections of the lungs, but in no case fatally.

The two months immediately succeeding the termination of the influenza were remarkable for salubrity. The predisposition to disease appeared to have been destroyed by the preceding epidemic.

Yours, respectfully,

JOHN STEARNS.

Dr. J. R. Coxe.

Observations on Jaundice, read before the Philadelphia Medical Lyceum, November 16, 1808. *By* ELIJAH GRIFFITHS, *M. D.*

MR. PRESIDENT AND GENTLEMEN,

BEING about to offer some observations on jaundice, I shall pass by such causes as are considered local, and confine my remarks to those only that are general.

In the course of these remarks, dropsy will claim some attention, and, from the relation it bears to the former disorder, it will of course be introduced.

There is certainly no better way of arriving at a rational mode of curing disorders, than by investigating and understanding their causes. This is part of my present object. It is a fact well known to physicians, that these complaints are almost always preceded by fever, or by intemperance in the use of ardent spirits.

In a few cases jaundice may appear without previous fever, or irregularity; but, with professor Rush, I would ascribe it to the cause of disease passing by the arterial system, and attacking the liver directly.

It is highly probable the celebrated Darwin was correct, in ascribing one kind of jaundice to a torpor of the liver; we can readily conceive, that the vessels of this very vascular and delicate viscus may become paralyzed by the stimulus of overdistention, which will always be the case in fever left to itself, or attempted to be cured by heating nostrums.

Although I cannot hesitate to believe in the existence of the elements of bile in the blood, yet I am not prepared to admit, that a suspension of action in the liver is the only cause of its being rendered sensible, for blood drawn from the vein of a healthy person exhibits no bilious appearance at that time, or any subsequent period; further, the most extensive inflammations, and destructive suppurations, as well as schirrhi of the liver, have existed without producing true jaundice.

We shall proceed next to inquire whether vicarious functions are not performed by certain parts of the body, under peculiar circumstances.

A fluid very much resembling urine has been discharged from the stomach*, at the time of the kidneys labouring under a suspension of their functions from disease; and a discharge of blood from the stomach has been frequently witnessed, at the proper period when the menses should have appeared, but did not; the same vicarious discharge has occasionally come from the nose, and from other parts of the body, in suppressed catamenia†.

Admitting the above position to be correct, we can readily conceive of the extreme vessels of every secreting organ of the body departing from health, by taking on the function of the liver, and separating at least the colouring matter of bile from the blood.

The bilious tinge of the serum of the blood is still unaccounted for; but when we view the immense changes wrought

* Mem. Acad. Royale des Sciences, 1715, p. 12.

† Boerh. Acad. Lect. vol. 5, p. 115 and 118.

on the blood by the sanguiferous system, in inflammatory and malignant disorders, we will not be struck with so much surprize at this system being capable, by a disordered action, of rendering sensible the latent elements of bile contained in the sanguineous mass.

We shall, therefore, consider the yellow tinge of the patient, the torpid bowels, and dyspeptic state of the stomach, as links in the chain of symptoms denoting a disordered state of the body, either dependent on the original cause, or on the torpor of the liver.

When, in these cases, the absorbents have become very inactive, and the secerning vessels of the arteries have assumed the nearly suspended functions of the kidneys and perspiratory vessels of the skin, we may look for dropsy to follow, with the utmost certainty.

It would be superfluous, in this place, to offer any arguments to prove that fevers and intemperance are the most common causes of jaundice and dropsy. We shall therefore proceed to inquire how they operate as causes.

It has been long known that stimulating remedies, or even a neglect of depletion in fevers, have produced these complaints. The great declension of the solids, and corresponding increase of the fluids, in fevers that have lasted some time, prove incontestibly the sanguifying power of the absorbent system. But the absorbents are not capable of taking up bones, flesh, or even fat, undissolved. We must then look for a solvent of these solids in an inversion of that power which originally formed them from the animal fluids.

What that power is, or on what actions of the animal economy the consolidation and solution of living matter depends, I confess my ignorance; however, we are sensible of the effects of this process in fevers, by the rapid increase of fulness in the sanguiferous system, so as to effect the changes above named. Hence the efforts of nature to relieve herself by hæmorrhagy, profuse sweats, diarrhœa, &c. which also prevent many dangerous terminations of fevers, unsubdued by the hand of art.

The due oxygenation of the blood is known to be essential to the healthy actions of the animal economy. This can hardly be expected to take place, when the transmission of blood through the lungs does not bear a proper ratio to the whole mass; for in the stage of undisturbed fever, tending toward a chronic state, we find the pulse quick and frequent, but small: now it is fair to conclude a correspondent action takes place in the heart and pulmonary arteries; an engorgement of the viscera, with deficiency of oxygenation, will be the consequence, and must have a most extensive influence on the vital functions of the body, in altering the appearance and quality of the fluids, diminishing the secretions, favouring effusions, and retarding the action of the absorbents, by choaking up the outlets of the thoracic duct.

By way of illustration, I beg leave to offer the following few cases, regretting that I have not had a more ample opportunity of deciding on the efficacy of the means, previous to laying them before the society.

In March, 1807, I visited Mr. M. who had for a considerable time laboured under a quotidian form of fever, for the cure of which he had made a very liberal use of Peruvian bark. This had diminished the violence of the paroxysms, but greatly

protracted their duration, so as to give the disorder nearly a continued form of fever, with a periodical sense of cold, but not a chill. The pulse was weak, but frequent and quick; he was free from pain, but very weak. Tart. antimon. joined with sulph. sodæ, were given to the extent of full vomiting; a blister was applied over the stomach; after which nitre, tart. antimon. and calomel were given till the mouth was affected.

The fever now assumed the quotidian form, with fair paroxysms and intermissions, and was to all appearance speedily cured, by the combined use of Peruvian bark and soda.

After a few weeks, I again visited this patient, and learned he had continued weak, with loss of appetite and constipated bowels, ever since I had left him. His skin was considerably tinged by bile, alvine evacuations of a greyish colour, bowels slow, pulse small and weak; he was free from any pain or uneasy sensation in the region of the liver. Blood-letting and calomel were prescribed; the bleeding was not done; the calomel produced no other effect than moving the bowels.

A variety of medicines were resorted to without any good effect, for the disorder daily grew worse.

With this state of things Mr. M. become discouraged, and had recourse to empiricism, which was tortured for a time, without any change, except the patient's being worse than when he began. Blood-letting was now used for the first time, with the most happy effect, for in less than twenty-four hours bile was discovered in the evacuations from the bowels; another bleeding sensibly diminished the yellowness of the skin, and one or two more removed the disorder, without any other remedy.

An indented servant man, belonging to Mr. Kraft, near Kensington bridge, was taken down with a violent bilious fever, in August, 1807. This patient sunk so much under the disorder that the lancet was but sparingly used. His convalescence was very slow, with slight occasional fever, debility, and very little appetite. This was soon followed by a tinge of yellow on the skin, and ash-coloured stools. A strong decoction of the Barbary bark was given freely, but the yellowness increased to the deepest tinge. I now had recourse to blood-letting, although the pulse was weak; two bleedings and a purge of calomel presented bilious stools. The yellowness soon disappeared, and the patient recovered perfectly without any other remedy.

Mr. F. W. a bricklayer, about thirty years of age, in January, 1807, complained of a pain in his side, with laborious respiration, which was much increased by a horizontal position; at the same time his abdomen was hard, and very much swelled.

He was advised, by some friend, to take a strong decoction of digitalis, which operated as an emetic and cathartic, and speedily removed the complaint. He remained well till about the end of June, 1807, when he became affected by a tertian fever, with very indistinct paroxysms.

He had medical advice at this time, but was not relieved by it; the difficulty of breathing returned, with swelled legs, and a hard tumour in the epigastric region. Application was then made to a notorious German impostor, who professed to cure all disorders by vegetable teas. This king of quacks amused him for some time with hopes of relief, but finally left him far worse than he found him.

On the 27th of November, 1807, I first visited Mr. W. and found his pulse very frequent, weak, and threadlike; his extremities cold, and very much swelled; respiration so laborious as to oblige him to sit erect night and day; his abdomen contained a large quantity of fluid, and a large tumour could be traced from under the scrobiculus cordis, below the umbilicus, and a little to the right side.

The urine was in small quantity, and very high coloured; bowels slow, appetite bad, with occasional nausea, and a dull pain about the stomach.

Calomel, nitre, and tart. antimon. were tried in small doses, but disagreed with his stomach so much that they were changed for a weak decoction of digitalis, with carbonas sodæ, which was also immediately rejected by the stomach.

After trying every formula of medicine I could devise, and each proving equally offensive to the stomach, and the system sinking under the disease, with a pulse scarcely to be perceived, and too frequent to be counted; cold, clammy sweats, and the breathing so obstructed as to give the skin a livid hue, I determined to unload the system by puncturing the legs and feet. These punctures discharged very freely, and gave considerable relief to the breast. A blister was applied over the stomach, and dressed with mercurial ointment, also ʒi of vitriolic ether was given every two hours. On my next visit I was astonished to find his pulse at sixty strokes in a minute, regular, somewhat full, but rather corded. The stomach was so well as to retain drinks, medicines, &c. The respiration was much less laborious, and the anasarcous swelling greatly diminished by the free evacuations from the punctures.

The ether was continued, but in smaller doses.

Blisters were applied to the legs; they, and the punctures, soon assumed a livid appearance, and threatened mortification; the yeast poultice reduced the inflammation, in two or three days, to the most simply suppurative kind; the patient had been for the last three days taking sp. nit. dulc. in parsley tea, which proved very diuretic. The dropsy was now entirely removed, and the patient approached nearer the figure of a skeleton enveloped in skin, than that of a man in ordinary health.

The pulse was small, but frequent and very tense; the excretions were in very diminished quantity and slow, unless urged by medicine. A yellowness began now to appear on the skin, with such excretions as are common in jaundice. A strong decoction of the berbaris vulgaris was administered, and continued for some days, without any benefit to the patient. The pulse continued small, corded, and about 90 strokes in a minute; the yellowness of the skin had arrived at the deepest shade.

From the good effects of blood-letting in the two former cases, I was induced to try it in this. The first bleeding produced no change, but the second did in the colour of the skin, and excretions; the third and fourth bleedings removed most of the yellowness and other symptoms, except a most incessant cough, which had recently come on, and soon disappeared by a copious expectoration. By the free use of mercurial ointment to the skin, and blisters, a gentle salivation was induced. The tumour in the abdomen disappeared, with every other appearance of disease but debility; when a course of gentle tonics was advised.

I now concluded my patient was quite well, but in that I was mistaken; for, on the 5th of January, 1808, Mr. W. complained of a pain in the region of the liver, head-ach, and some oppression at the breast; his feet and legs began to swell considerably, and his pulse was more than usually corded. I drew ℥x of blood from his arm, and directed sp. nit. dulc. in parsley tea. On the 6th and 7th, his pulse was very irregular; he was bled each day. By examining the abdomen, I found the spleen hard, and very much enlarged; a blister was applied over it, and dressed with mercurial ointment. This had a very good effect in dispersing the tumour.

Between this and the 15th, several diuretic medicines were taken, but without any benefit; the pulse was generally about 95 strokes in a minute, small, and a little tense. The 15th January I examined his abdomen, and found his liver very much enlarged and hard; nitric acid was administered very freely, and continued till the 19th instant. The pulse was 110 in a minute, with an aggravation of all his symptoms; ℞. pulv. fol. digital. gr. ij cal. ppt. gr. iss three times a day. 23d inst. his mouth was sore, pulse 100 and a little corded, but fuller; tumour in the abdomen diminished in size, and the other symptoms all abated.

From this till the 31st, he took his medicine in less quantity, on account of sick stomach; I found the salivation alternate with severe pains of the bowels several times.

His pulse was reduced between the 19th and 31st, from 110 to 60 strokes in a minute. February 2d, the pulse was at 40 strokes in a minute, and regular. The medicine was omitted till the 4th, when the pulse had risen to 60. He was directed

to take one powder every night. The pulse rose to 120 the 11th, but sunk to 84 strokes by the 16th. Every symptom of indisposition had disappeared; the pulse was soft, regular, and fuller than at any previous period.

Mr. W. continued to mend from this time, and I had the satisfaction to see him in the enjoyment of perfect health last August.

I have been thus particular with the last case, in order to show how low a patient may sink in these complaints, and still be restored to health.

Woodhouse's Reply to Seybert's Strictures on his Essay concerning the Perkiomen Zinc Mine.

To the Editor of the Philadelphia Medical Museum.

SIR,

IN a work dedicated to the interests of science, it ought certainly to be expected that those whose leisure or opportunities permit them, occasionally, to throw in their contributions to the general stock of knowledge, would discard every thing like *asperity* in their remarks on the opinions advanced by others; and that the *little* passions of *envy* and *jealousy* would never actuate the minds of those, whose *real* object is the pursuit of truth.

It was, therefore, with surprize, mingled with regret, that I perused the paper inserted in your last number by Dr. Seybert, on the subject of the Perkiomen mine.

As it is of little consequence to the public whether or not Dr. S. knew that *blende* or the *sulphuret of zinc* was found near Perkiomen in 1806; and, as it is equally immaterial, whether or not, in 1807, when shown a specimen of this same ore, he declared it to be *lead* ore; I shall proceed to show that his essay, improperly entitled "Facts [when it entirely consists of quotations] to prove that this metallic ore can be worked to advantage in the United States," proves nothing, except the doctor's misplaced rancour against myself, and which my former essay has furnished him a pretext for exhibiting.

Without entering into a comparison of the doctor's patriotism with my own; without pretending that my attachment to my *natale solum* is as strong as his; or that I should be disposed to make as great sacrifices, either personal or pecuniary, for my native country as Dr. Seybert would, I shall show,

1st, That there is an evident *want of candour* in the conclusions he has drawn from my publication.

2d, That some of his quotations from chemical writers are unfairly given.

3d, That what he has advanced bears no direct relation to the subject in question.

4th, That the observations in his concluding paragraphs are *highly personal* and improper; and

Lastly, I shall annex *correct* extracts from the best modern writers to show that the *blendes*, though they abound, are seldom worked in Europe.

In the first place there is a want of candour, because the doctor asserts that I have maintained that this American ore will yield 72 per cent. of metal; whereas I expressly mentioned that this quantity was *not* given as *accurate*, from the difficulty of analyzing the ores of *zinc*, and the reasons are assigned.

An erroneous conclusion is drawn from what I have published on this subject. In order to place this in a clear point of view, I will here insert the two paragraphs which have so much excited the irascibility of the doctor, and let the reader compare them with his remarks.

"Can this ore be worked to advantage in the United States?

"No information on this subject can be obtained from any book with which I am acquainted. Mr. Meade, a gentleman possessed of extensive knowledge on mineralogy, informed me that it is never worked in England. Dr. Bruce, professor of this science in the College of Physicians, New York, told me that it is reduced in Wales; and Mr. Godon, of Boston, who is extremely well acquainted with subjects relating to this business, has declared that the zinc cannot be obtained from this kind of ore, but with the utmost difficulty."

For thus merely stating the information derived from three men of eminence, without advancing any opinion of my own, Dr. Seybert has taken the liberty of asserting that I assume the principle that blende is not and cannot be worked *any where* to advantage.

Secondly, The quotations from some writers are not fairly stated. This will appear from the following extract from Dr. Seybert's essay, when compared with what has been said by bishop Watson and the celebrated Chaptal, on the same subject.

"At Rammelsberg, near Goslar, there is a considerable manufacture of brass. I visited it in 1794. Here they form this important alloy with (cadmia) a sublimed oxide of zinc which is obtained by proper management during the roasting of the lead ores and blendes, in a reverberatory furnace."

Here an incorrect idea is held forth, that a mine of *blende* is worked near Goslar in Germany, but the fact is, that the mine at that place is wrought for the *lead* and *silver* it affords; and, as no additional expence is incurred from the purchase of fuel, the oxide of zinc is obtained at the same time; and the mine is a *lead*, and not a *zinc mine*.

Now let us examine what Chaptal and bishop Watson actually do say on the subject, when divested of the doctor's *alloy*.

Chaptal, vol. 2, *p*. 46, 4*th Amer. edit.* "Zinc is sometimes mixed with lead, and in the working of this last metal, the former is *occasionally* obtained. Such is the ore worked at Rammelsberg, near Goslar. Great part of the zinc is dissipated, but a *portion* of this metal is obtained by a very ingenious process."

Watson's Chemical Essays, vol. 4, *p*. 40. "At Goslar, in Germany, they smelt an ore which contains lead, silver, copper, iron, and zinc, in the same mass. The ore is smelted to

procure the *silver* and *lead;* but, by a particular contrivance, they obtain a portion of zinc in substance."

The slightest observation will show that Dr. S. has taken as much from bishop Watson's work, as would suit his purpose, without any regard to conveying the true meaning of the author.

It is true, the bishop says, vol. 4, p. 20, " The sulphuret of zinc has for many years been used, as well as calamine, for the making of brass at Bristol ;" but, in p. 40 of the same essay, he informs us, that " as to this ore of zinc, it is not so commonly used as calamine, for the making of brass at Bristol. Several ship-loads of it were sent, a few years ago, from Cornwall to this town. Upon the whole, however, experience has not brought it into reputation at this place."

But Dr. S. infers, that because a lead mine in Germany, which is worked for the silver and lead it contains, affords a *portion* of zinc, that therefore a zinc mine, containing little lead and no silver, can be worked to advantage in the United States.

Thirdly, What the doctor has advanced in near eight octavo pages, bears no direct relation to the subject in question. It is not by quotations from foreign writers that we can determine whether metallic zinc, or any substance into which it enters as a component part, can be worked with advantage in the United States : but by taking into consideration the extent of the ore ; the quantity of metal it will afford when worked on a large scale (which can only be ascertained by an experiment with several hundred weight of the ore) ; the price of labour in this country ; the cost of fuel to throw off the sulphur, and

afterwards to extract the metal; the demand for the zinc, and the price for which it can be manufactured abroad and imported into this country. Not one of these circumstances have been considered by Dr. Seybert; and yet, with a kind of self-complacency, as if the consideration of these points was beneath his dignity, he says, "I DO MAINTAIN that the Perkiomen blende can be worked in America with advantage." There is a trifling difference, however, between *assertion* and *proof*.

The question is not whether the blendes are, or are not, worked in *Europe?* but, whether we can manufacture metallic zinc, or compounds into which it enters, cheaper than they can be imported from England, France, Germany, and the East Indies.

Calamine, an ore of zinc, which can be easily and profitably wrought, abounds in Great Britain. "As we have greater plenty of calamine in England," says bishop Watson, "and that of the better sort, than most other nations have, there is no fear of our losing the advantage in this article of trade, which we are now possessed of." *Essays*, *vol.* 4, *p.* 7.

The blendes of Perkiomen also differ, *materially*, from those of other countries; and they all differ from each other.

"The nature of the sulphurets of zinc," says *Fourcroy*, *vol.* 5, *p.* 519, "is not well known, and Bergmann found such great differences between them, from different countries, that the ores seemed to possess no identical properties, or composition."

Fourthly, The observations in the concluding paragraphs are highly personal* and improper. Although I had advanced no opinion, the doctor has undertaken to *denounce* me as one "exciting unfounded doubts, propagating erroneous opinions, and *attempting to paralize* the wise efforts of a judicious public," (although no efforts have ever been made by the public, to work the Perkiomen mine), "and at a time when the political situation of our country is such, that its foreign relations are interrupted, and much is expected from an energetic application of its internal resources."

In France, during the gloomy periods of the revolution, such a denunciation would have been sufficient to bring the object of it to the *guillotine;* and, *were it true*, it ought, even here, justly to excite the hatred of my fellow-citizens against me. But I repel the insinuation with the contempt it deserves. In every analysis I have made, public utility has been my sole object, and to that object my attention has always been cheerfully devoted, without any regard to labour or expence.

Correct extracts from the most celebrated modern writers are annexed, which prove that the blendes or sulphurets of zinc are *seldom* worked in Europe.

The abbé Hauy, whom Fourcroy, the great and enlightened historian of chemical science, very justly styles "the last, most

* The Editor in justification of himself must remark, that however the paragraphs may have appeared to Dr. W. to himself they conveyed no marks of personality, which would have precluded the admission of the communication. He trusts that no further remarks, which can in the remotest way be viewed as personal, will be offered for insertion, as it is the furthest from his wishes to encourage any thing of the kind, although a fair and candid discussion of any subject will always meet his ready attention.

learned and accurate author on mineralogy," after mentioning that the sulphurets of zinc abound in the mines of Saxony, Bohemia, Hungary; that they are found in Sweden, Norway, England, France, &c. and that, in general, zinc is one of the most common of the metals, informs us, that "this metallic substance is scarcely an object to seek after, and that it is casually extracted in the melting of minerals with which it is associated, and particularly the sulphurets of lead*."

"The sulphurets of zinc are scarcely worked by themselves, or with the sole intention of extracting the metal. It is most frequently by fusion with the ores of lead, mixed with sulphuret of zinc, that the latter metal is obtained." *Fourcroy, vol.* 5, *p.* 522.

"Blende is sometimes, although extremely rarely, worked as an ore of zinc." *Jamieson's Mineralogy, vol.* 2. *article Zinc.*

"Zinc is obtained *adventitiously*, in the melting of such copper and lead ores as contain zinc or blende." *Weigleb, p.* 419.

"*I do not know any country where blende is wrought to obtain zinc.*" Chaptal, Amer. edit. vol. 2, p. 46.

"As the consumption of zinc is very limited, it has not been much worked." *Chemistry applied to the Arts and Manufactures, by Chaptal, vol.* 2, *p.* 209.

* "Cette substance métallique n'est guère un objet direct d'exploitation. On l'extrait accessoirement par la fonte des mines auxquelles elle est associée, et en particulier du plomb sulphuré."

Mineralogie, par Hauy, tom. 4, *p.* 170.

"Calamine is the ore of zinc that is always worked. The extraction of zinc from blende is attempted, but not often." *Murray*, *vol.* 3, *p.* 34.

"*No mines are worked in order to extract zinc.* In fusing lead ore, mixed with blende, the metal is obtained in the state of an oxide." *Lagrange*, *vol.* 2, *p.* 34.

I might swell this list of quotations with extracts from other eminent chemical authors; but I shall forbear, as I should, no doubt, meet with this *conclusive* answer from Dr. *Seybert*, that "*Those authors do not know all that is done in this way!**"

The reason that the blendes are not worked, is the great difficulty of throwing off the sulphur they contain. The following experiments have lately been made:

One pound of the Perkiomen ore, reduced to a fine powder, was exposed eight hours in a blast-furnace to the heat of the Lehigh coal, which is much greater than can be excited by any other kind of fuel, and it lost only three ounces in weight. Aqua regia was then added to an ounce of it, and a quantity of sulphur immediately separated and swam on the surface of the fluid. When washed and dried it weighed 150 grains.

Eight ounces of this roasted ore, mixed with the powder of charcoal, were submitted in a proper manner to the same heat for ten hours, and an inconsiderable quantity of metallic zinc was procured.

An unsuccessful attempt was also made to manufacture the sulphate of zinc, or white vitriol, in the same manner as they

* Seybert, p. 211, Medical Museum, vol. V, No. 4.

do in some parts of Europe, by submitting a lump of the ore, weighing two pounds, to an intense red-heat, repeatedly extinguishing it in water, and evaporating this fluid to dryness.

As Dr. S. has made no experiments on the Perkiomen ore, it is absurd for him to pretend to give information to others on this subject, when he possèsses none himself.

For my part, I would sincerely rejoice to see this or any other metallic substance wrought *to advantage* in this country: coinciding freely in the opinion of the illustrious Chaptal, "that although AGRICULTURE is the basis of the public welfare, the ARTS and COMMERCE form the *glory*, the *ornament*, and the *riches* of every polished nation."

I shall now conclude my reply to Dr. Seybert by introducing a letter on this subject, which has been addressed to me by a mineralogist, who, in that science, is second to none, either in this or any other country.

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SIR,

THE question at present between you and Dr. Seybert is, whether it be possible to use, *with advantage*, the blende of Perkiomen in the manufacture of zinc and brass. It appears to me that the solution of this question is beyond the limits of chemistry and mineralogy, and becomes a question merely of speculation. On this point, I think the quotations from European authors perfectly useless, for none of them declare positively that any benefit has resulted to those who have tried this experiment. Besides, their authority must pass for nothing as it regards America, where circumstances are so dif-

ferent from those of Europe. I have no hesitation in giving my opinion against a manufacture intended for the transformation of *blende* into *artificial calamine*, in a country where I am as yet unacquainted with any mine of copper being in *actual exploration:* but my idea on this subject may be susceptible of some modification, as I am not sufficiently advanced in a knowledge of the country, to have ascertained the price of various articles necessary to the manufacture, and to render it really serviceable.

Dr. S. anticipates the near approach of a time, when we shall see the articles of zinc and copper, as forming interesting items in the list of articles exported from the United States. If this should prove correct as to *brass*, it certainly never can be so as to *zinc*. I have always seen that metal at so low a price, and in so little demand, in Europe, that it is very doubtful whether that part of the world (the only place to export to) will ever present an advantageous market for zinc *manufactured in America*. But if, in fact, Dr. Seybert has a good opinion of the utility of exploring this mine, a natural *proof* presents itself for corroborating his own opinion, and proving that what he advances to the public is the *fruit of reflection*. It is said he is wealthy: let him furnish the funds, and his information, towards an object of which he has so high an idea, and which ought now to afford a greater prospect of success, as a mine of copper, of which *he had no knowledge at the time of publishing his essay*, has just been discovered near that containing this blende. For my part, I sincerely hope this business may succeed; but so far I see no reason to change my opinion.

Observations intended as a support to the Doctrine of Cuticular Absorption, and in opposition to opinions of late advanced on this subject. Read before the Medical Lyceum, February 22d, 1809.

IT is not my intention to enter largely into the subject of absorption, nor to consider the question still proposed by the Lyceum as a fit subject for a premium, whether "the human body possesses the power of absorbing substances applied to its surface?"

It will readily be admitted that it is easier to object to an established hypothesis, than to raise another free from all objection, in its place. Yet, however I may estimate the very ingenious experiments of the different authors on this subject, I cannot but say that my mind is not converted to their mode of thinking; nor indeed am I able to give up to their forcible arguments the plain evidence of my senses.

My chief inducement, in the present instance, is to oppose a doctrine which I perceive is upheld by the opponents to cuticular absorption, viz. that this function is carried on by the lungs, which are *supposed* to receive the volatile emanations through the medium of respiration; the facts and arguments to prove which, allowing every force which can be demanded against cuticular absorption, appear to me so vague that I cannot withhold my dissent from it. Should future experiments demonstrate fully that the skin is not the grand superficies whence absorption takes place, I must still dissent from allowing the lungs more than a partial right to it, if indeed they come in for any share whatever of the function.

I shall here observe that the opponents to cuticular absorption admit this function taking place, provided the cuticle, that seven-fold shield, be removed ; but they leave us totally in the dark as to the use of the numerous lymphatics which are spread over the surface of the body. Now, as nature certainly never intended the removal of the cuticle in a healthy state ; and, as it is clear that so large a congeries of vessels could never be proposed for no use, I think we are warranted in demanding of the gentlemen, what purpose they are intended to answer ?

It appears to me, that from the general tenor of the experiments, and the substances employed, the gentlemen consider the lymphatics as solely an appendage to the urinary bladder, as we find them almost constantly attending to that viscus, to discover in the urine the presence of the articles employed, whether those experiments be intended to disprove absorption from the surface, or to assert the dependance of this function upon the lungs. It is true, that they are presumed (at least I suppose this is the presumption) to convey their contents thither through the medium of the blood-vessels ; but can we not safely suppose, that however the presence of some articles, as turpentine, may be detected in the urine, there are others which are conveyed by the same channels to other emunctories of the body, without having any aptitude to be discharged by the bladder ? If so, the absence of any particular substance in the urine certainly cannot disprove its being conveyed by absorption from the surface.

It is very surprizing to me, that with the absolute certainty of absorption going on in the stomach and intestines, these parts have never had, at least, a portion of that power ascribed to the lungs assigned to them. Certainly their right in an

equal, if not superior degree, cannot be denied: but it would seem that the opponents to cuticular absorption, so long admitted, appear to think, unless they point out an absolutely new channel for this function, to the total exclusion of the skin and alimentary canal, they do nothing. But it will not be denied me, that the stomach and intestines* possess every power as it regards this function, which they so sedulously ascribe to the lungs solely. Every experiment they detail, proves this sufficiently; for it is not possible to convey any thing into the lungs, a part of which, inviscated in the saliva, shall not descend into the stomach. If then, I say, their experiments disprove, *in toto*, the absorption from the surface, they must at least admit the stomach and intestines to an equal share with the lungs. But, as I consider it as a more just doctrine, I must regard the former as possessing entirely this function, if cuticular absorption be at any time overturned.

In opposition to that doctrine which dissents from cuticular absorption, I shall remark, that, in my opinion, experiments founded on the external application of spirit of turpentine are very ill adapted to disprove the point aimed at. Without giving to the lymphatics the power of election, it appears very unlikely that these minute vessels could in any case be induced to take up an agent so acrid and stimulating, that we know of

* After all, the stomach, &c. are still a part of the exterior of the body, and differ only from that generally so esteemed, by the coating of cuticle; the action of this amazing congeries of vessels being unique, whether on the external or internal surface, or within the cavities of the body. Must we then actually suppose, that the mere appendage of cuticle overpowers the function appertaining to the lymphatics! When we see the function of perspiration still provided for in spite of a cuticle, would it not better become us to presume, that, by some hitherto unexplained contrivance, the cuticle is in fact no more an opponent to the entry than to the exit of fluids?

none better adapted to constringe vessels to which it is applied; and hence its great utility in stopping hemorrhagies. Now, if vessels so much larger are coerced to close upon its application, even when aided by a discharge of their fluids, it surely requires little reflection to perceive, that on the more minute and delicate fibres of the lymphatics, this effect must be still more powerful. Hence then, I say, these and similar experiments can never be adduced against cuticular absorption with any success; and, although the same arguments may be urged against the lymphatics of the stomach and intestines, equally with those of the lungs, although fact evinces its presence in the urine after inhalation, yet I must contend that there is a wide difference between the palpable application of the substance itself to the external surface, and that which arises from its mere volatile and finest parts inviscated in the saliva, and carried into the stomach*.

It may be said that the effect perceptible in the urine is too rapid to explain it by this circuitous route; but, does not the same objection, in a degree, hold good as it applies to the pulmonic theory of absorption? But the experiments of Mr. Home, lately published, give us a very ready explanation of this rapid passage to the bladder, and confirm the doctrine I have advanced, and which I have many years upheld, al-

* One of the first and most indefatigable opponents to cuticular absorption, in denying the conveyance of simple fluids into the system, through this channel, ascribes the prevention or cessation of thirst, by the application of wet cloths to the skin, to the constriction of its pores, by which perspiration is diminished. If so bland a fluid as water could not gain admittance, surely experiments by a fluid so much more acrid, might be deemed useless. But if the theory be just, whence the thirst in fever, when perspiration is so greatly obstructed? and whence the immediate relief from thirst, when a free and profuse perspiration is by any means excited?

though it has never yet, that I know of, been adverted to by the graduates of this university, in their inaugural dissertations upon this subject.

In referring to the experiments detailed in the essays I have read on the subject, I find that the turpentine, in every case when the violet smell was perceptible in the urine, was inhaled, or in other words was carried both into the stomach and lungs (for I consider it as absolutely impossible that some portion should not be carried into the former viscus entangled in the saliva); whilst, on the other hand, when the violet smell was not sensible, there an exclusion took place equally from both organs. Hence, the gentlemen cannot urge an objection against the absorption of the one, which is not equally conclusive against the other; nor advance a fact which will not be as favourable for my opinion as for theirs.

I have said that Mr. Home's experiments very satisfactorily explain, and without the aid of Dr. Darwin's retrograde absorbents, the rapid manner in which fluids, &c. are conveyed from the stomach to the bladder of urine. This celebrated anatomist found that the fluids are principally contained in the cardiac, or left portion of the stomach; whilst the more solid contents are consigned for digestion to the pyloric extremity; the fluids, above what are accessary to digestion, being carried out from the stomach, without ever reaching as far as the pylorus. A series of experiments convinced him this was effected by vessels passing from the stomach to the spleen. In some of these experiments, the effects of articles taken into the stomach showed themselves in the urine, within a quarter of an hour after being taken (the period at which the most early detection of spirit of turpentine has been made in that fluid).

Another fact, confirming the first, and explanatory of a circumstance I was until now unable to account for, is, that after some time the effect on the urine was no longer evident; but at a more distant period of nearly seven hours, it became again sensible in the contents of the bladder.

Here then we see a portion of the substance employed (rhubarb) rapidly passing off with the fluids from the stomach, and exciting evidence of existence in the urine, by the route through the spleen ; and at a later period, when we may suppose another portion had passed with the chyle into the intestines, and thus, through the more distant course of the lacteals, again evincing its presence in the bladder.

The fact I allude to, as now explained, is that I have repeatedly detected, on eating asparagus, the peculiar unpleasant odour it produces in the urine, in a very short period after my meal. This, in a few hours, entirely subsided, but recurred after a longer interval, in which that portion not taken up immediately from the stomach to pursue the route by the spleen, followed the more circuitous one through the pylorus and lacteals.

By these experiments we can now readily explain the rapid excitement of urine on eating water-melon, and the frequent call for micturition when drinking freely after dinner, or on taking punch, or other articles favouring the secretion of that fluid. We see also a reason for the quick subsidence of those unpleasant feelings excited in the stomach by a copious meal, and which certainly the stomach could not, without hazard, contain entire, until carried off by the slow operation of digestion ; if indeed digestion could be at all performed when so large a mass had over-distended the stomach, and probably, like

the over-distended bladder, to a certain degree paralized its powers of action. Fearfully, indeed, and wonderfully are we made, when such various channels of preservation from destruction so clearly point out the superlative wisdom of a Divine Architect.

To the objections made by the opponents to cuticular absorption, to the introduction of mercurial ointment, by the external lymphatics, I can only say, that the opposition to a fact, or the failure of detecting the presence of that substance in the fluids, can only be regarded as negative, not positive proof. What changes may be induced by the action of living matter, either in a mechanical or chemical view, so as to prevent its detection by common tests, I know not. But, until I see stronger reason to doubt the evidence of my senses, than these gentlemen advance against its absorption, I must be allowed to deny my faith to the extraordinary manner by which they suppose it to be introduced into the system, viz. by being carried in a state of volatilization into the lungs. I must confess I regard this opinion as savouring too much of the straining at a gnat and swallowing a camel. I certainly may be allowed to demand of them their proof of this assumed fact. Is it not a mere petitio principii, strangely urged to favour their hypothesis? "Give me a place to stand on," says Archimedes, "and I will move the world." So they, "Grant me this, and I will readily prove the other." But assertions are not proof, and proof I ask, that mercurial ointment is ever volatilized by the degree of heat excited by merely rubbing it on the surface. By exposure of ℥i of strong mercurial ointment for two hours and a half to a temperature of 120° (a degree of heat, I apprehend, seldom exceeded by the friction of the hand on the skin), I found it lost only 15 grains, arising most probably from a loss of the sebaceous matter, and not of the metal. Again, let

these gentlemen, after guarding both hands with tight bladders, endeavour to produce the volatilization of the ointment, and tell us at the end of the experiment how much loss has been actually sustained in the ointment, after deducting the weight acquired in the process by the bladders themselves. I suspect the result will differ much from the same process carried on upon a naked limb. How is it also that we safely employ mercurial frictions for another, in large quantities, when guarded with a bladder, which shall nevertheless excite ptyalism in the patient, if the effect was derived solely from its volatility? Ought not *both* to be equally affected? I know the reverse has been stated, but certainly one solitary negative fact is not to overturn the host of proof in the opposite scale. By requiring too much, we often fail in establishing what we desire. If it is not carried in by the cuticular absorbents, it certainly is not by the way pointed out by these gentlemen; and my intention here is to attempt to overturn the foundation they have lain for a new superstructure in the system of absorption.

I would here recommend the favourers of this volatile theory to put it to the test, by suspending a piece of gold directly over the limb on which the ointment is rubbed, or in the centre of the venereal ward in the hospital, and inform us how long it actually takes to induce any appearance of amalgamation on it, visible by the volatilization of the mercury.

I shall here mention my opinion, that I think the smell of the turpentine in the breath, no more proves that it is taken in by that channel, than the smell of violets in the urine proves it to be absorbed by that viscus. The same vessels which convey the odorous particles from the stomach into the blood-vessels, give those blood-vessels, by the medium of the circulation, an equal chance of conveying them to the bladder and to the

lungs, and this is in a great degree proved to be the case, in some experiments detailed, where injections of turpentine were employed, and where its presence was soon after detected both in the urine and in the breath. Now certainly, as inhalation was particularly guarded against, I think all chance of absorption was completely cut off from the lungs, and its presence there could arise only from absorption in the alimentary canal, and conveyance to the lungs by the medium of the blood-vessels.

If I am here questioned relative to the various other articles employed in the experiments, I can only say that I have not yet fully considered, and must therefore be excused from a present reply. I have taken the two substances principally adverted to, and think I have fully established the point that turpentine never is, nor can be taken up by the cuticular absorbents, whose action nevertheless I consider as absolute; and that mercurial ointment never is, nor can be volatilized by the temperature employed upon its use; consequently, that as it disappears, it is actually taken up by the cuticular absorbents. I think I have also proved that the stomach, and not the lungs, is the viscus to which we are to ascribe the introduction into the system of those articles which nature never proposed, from their acrimony, to find a passage through the delicate superficial lymphatics, but which, by impregnating with their minute and volatile particles the food and saliva taken into the stomach, are thus enabled to evince their presence in very different and distant parts of the body. I have, therefore, if cuticular absorption is not admitted, at least done something in preparing the road for further inquiry.

Philadelphia, February 18*th*, 1809.

Observations on Anomalous and Irregular Diseases.

By Dr. G. WILLIAMSON.

Baltimore, November 2, 1808.

AMONGST the many interesting medical essays which are presented us through the medium of the various periodical publications, we are rarely favoured with any account of those irregular diseases which all physicians of extensive practice must occasionally meet with. These diseases, when violent, are generally very perplexing; to the young practitioner they are inexpressibly so: to him they are new, and he is at a loss with what order, genera, or species of disease to class them; and, should he have even thrown off the shackles of nosologia, they are still very perplexing. At one visit the disease appears in one form, at the next in another. At one time one viscus seems principally affected, and at another some other. Diseases of this nature cannot be reduced to any systematic order; the doctrines of diagnosis and prognosis are here equally futile. Brown's system is too simple, and Darwin's too abstruse; each would admit them within their classifications, but neither would enable us, in many cases, to speak intelligibly. When this is the case, I know nothing better that we can do than, as Rush says, prescribe for symptoms, without being solicitous to give the disease a name. However, I think, if more attention were paid to these diseases, and they were faithfully detailed, it would be of singular service to the practitioners of medicine in general, and to the younger ones in particular.

Senac, in his invaluable Treatise of the Hidden Nature of Intermittent and Remittent Fevers, has given much very interesting information of those diseases under an irregular type.

When I first read his work, it was neither interesting nor pleasing to me; but in the autumn of 1807, after the influenza, I met with many cases of disguised intermittents, and, as soon as they appeared, I remembered what Senac said, and was much gratified, that a perusal of his work had enabled me to recognize immediately the disease. We never know how to justly appreciate any thing until we experience the want of it. So it is with respect to information. Such was my situation in regard to Senac's treatise; and had I not read it, should, with many of my fellow-practitioners, supposed this disease some other than it really was; and I do not believe that any practice would have been equally successful as that which I adopted, and which was suggested in consequence of the information derived from the perusal of Senac's works. Every case which I met with was soon remedied by the medicines proper in regular intermittents.

The causes of intermittents are generally known. In addition to the most common causes of that disease, there were, in 1807, concomitant ones. Whatever the cause of this disease may be, whether marsh miasma or any thing else, we know that, previous to its attack, there is a deficient excitement, and that the disease is of an asthenic order. Every person whom I saw labouring under this disease had previously the influenza; and all who had been thus affected complained of great general debility. In this city the influenza was less inflammatory than I suppose it to have been in many parts of America, consequently there was less occasion for much depletion. On another occasion, I have given it as my sentiment, that the too free use of the lancet frequently predisposed to the disguised intermittent.

Symptoms. The symptoms of the disease in question were very irregular, both in their appearance and in their time of appearing. There were some exceptions to this general rule. It most generally commenced with a slight chill, succeeded by excruciating pain, and terminated with a perspiration. No part of the body was exempt from pain, but the head was most frequently affected. In some it was confined to the forehead, in others it extended over the whole head. In some the occiput, or back of the head, extending down the neck, and in others one or the other of the sides of the head were only affected. In others the extremities, the sides, or the breast, were the suffering parts. In some cases, these pains only remitted; indeed, in a few cases, they were almost continual. At no stage of the paroxysms was there scarcely any increased arterial action, or other ordinary symptoms of fever; and that stage, which is generally termed the hot, should in this disease be termed the painful. From the disease frequently putting on a periodical type, although it was not very well marked, yet I was induced to suspect it a species of intermittent; and I was also induced to believe those of the same nature, whose prominent symptoms were analogous, even if they were less distinctively marked; and from there being little or no increased action in the pulse, from a perspiration succeeding when the pain either remitted or intermitted, and, finally, from the admirable success from the exhibition of bitters and tonics, I conceive my conclusion incontrovertible. Although the pain was in some cases so violent as to almost distract the sufferer, yet there seldom or never appeared in the parts affected any thing like congestion or local inflammation.

Treatment. Vesicatories were applied in a few cases, but their effect was very limited. Æther, laudanum, &c. as local

applications, had also a temporary effect. Those diffusible stimulants, which frequently break the chain of regular intermittents, were less efficacious in this disease; but bitters and tonics, when properly administered, never failed to cure.

We will now proceed to give a narration of the case which induced me to commence this essay.

February 15th, 1808, V. H. had for some time laboured under a severe pain in the left hip and lumbar region. This affection became so excruciating as to induce him to apply for medical aid. The physician who first saw him bled him and gave some powders, which I supposed to be a composition of antimony, &c. and directed a blister to be applied. But, in consequence of a severe pain immediately attacking the breast, with other acute symptoms of pneumonia, the blister was applied to this part, and he again bled; the powders continued. When I first saw him, the general symptoms were favourable: pain had nearly subsided, was nearly free from fever, skin moist, cough troublesome and dry. Tart. antim. et nitras potas. were given *ter in die:* this preparation kept up a determination to the skin, and also acted as a good expectorant.

16th. Was free from fever; pulse natural; bowels regular; there was a paucity of urine, with pain in passing it, occasioned, probably, by the vesicatory.

About the 20th, an affection of the stomach took place, resembling the pyrosis; there was also, at times, a discharge of blood from this viscus. As the aqueous discharge from the stomach was very considerable, to the amount of two or three quarts in twenty-four hours, I feared it would much debilitate my patient; consequently thought proper to arrest it: for

which purpose sal. tart. and a cold infusion of flor. cham. were given. Soon after commencing with these remedies, the discharge decreased ; as soon as it had nearly ceased, a sense of tightness through the chest, with a disagreeable sense of distension and weight through the alimentary canal, supervened. From the time I first saw him, he had complained of similar sensations, when his bowels were not evacuated at least twice in twenty-four hours ; when he had two or more dejections in a day, he felt light (to use his own expression) ; and when the affection of his stomach was suffered to return, it had a similar effect. It was not, therefore, desirable to stop it entirely, but to regulate it as the more important symptoms might indicate. For several days he was apparently much better ; but as these symptoms declined, the pain in the hip and small of the back increased. He was now very solicitous to have a blister applied ; from some disagreeable symptoms which occurred immediately after the application of the others, and which I supposed in consequence of it, I prevailed on him to defer this for a day or two. But the pain becoming exceedingly tormenting, and every other remedy proving ineffectual, I consented to the application of the blister.

26th. Called to see him before the blister was applied. The pain was excruciating ; he had just been puking a large quantity of blood. There were other alarming symptoms. There was more appearance of inflammation than there had been for some time, but not enough to warrant bleeding, or other active depleting remedies, in his very reduced state. From the then prevailing symptoms, I feared a nephritis. The emp. episp. was applied, and he directed to take the antimonial powders.

27th. Irritability of stomach had subsided, and the discharge from it nearly ceased; the powders had excited a diaphoresis; blister drew well; had no pain in the back or hip. There was a troublesome ischuria; for which the following ischuretica was prescribed, and directed to be given as occasion might require:

℞ Tinct. opii.	ʒi.	
Gum. camph.	℈i.	
Vin. antim.	ʒi.	
Aq. font.	℥ij.	M. f.

This preparation had the wished-for effect.

Occurred about this period a swelling in the left thigh and leg, accompanied with inflammation and great pain. The calf of the leg was very hard, and had much the appearance of an incipient suppuration. A solution of sacch. saturn. was applied.

29th. These symptoms had considerably subsided, and, in a few days, every appearance of suppuration ceased. The swelling continued. Stimulating washes were used to excite the absorbents to perform their proper functions: they had the desired effect.

Some months previous to this attack, V. H. received a hurt in the lumbar region; and he is naturally of a very delicate health.

G. WILLIAMSON.

Meteorological Table; and Statement of Deaths, with the Diseases and Ages, in the Borough of Harrisburgh, from the 1*st of January until the* 1*st of December*, 1808. *By Dr.* Joseph Kelso. *Communicated to the Editor by Dr.* James.

It is to be regretted, that a prevalent error seems to be established in the public opinion, respecting the health of this place. Whether propagated through mistake or design it matters not; but so the fact is. It is to be hoped, the following statement will convince the public of their error, and place us in a situation as congenial to health, as any other in the United States. I hope, at least, it may have the desired effect, which is the only inducement I have for laying it before the public.

	Thermom.			Barometer.	
August, 1808.	Morning	Noon	Evening	Degrees A. M.	State of the Weather, A. M.
1	58	61	61	29.4	Clear
2	59	64	63	29.3	Foggy
3	59	65	63	29.4	Clear
4	62	65	63	29.4	Sunshine
5	62	63	61	29.3	Sultry
6	50	61	60	29.4	Cool
7	59	58	58	29.4	Rain
8	59	59½	59	29.2	Cloudy
9	59	58	59	29.3	Do.
10	58	52	60	29.3	Foggy
11	59	64	62	29.3	Do.
12	60	64	64	29.3	Cool
13	61	68	62	29.2	Cloudy
14	61	64	63	29.½	Do.
15	58	60	59	29.5	Clear
16	57	58	58	29.5	Sunshine
17	57	60	59	29.5	Foggy
18	58	61	60	29.4	Clear
19	58	62	60	29.3	Foggy
20	60	60	60	29.3	Clear
21	60	60	60	29.3	Foggy
22	58	62	61	29.5	Clear
23	58	64	63	29.4	Foggy
24	58	63½	63½	29.4	Do
25	62	63	62	29.3	Sultry
26	62	63	62	29.3	Windy
27	62	63	62	29.2	Do.
28	63	62	63	29.3	Heat
29	57	61	60	29.4	Foggy
30	58	60	60	29.5	Cold
31	57	59	60	29.6	Cool

	Thermom.			Barometer.	
September, 1808.	Morning	Noon	Evening	Degrees, A. M.	State of the Weather, A. M.
1	55	60	60	29.5½	Cool
2	57	61	60	29.5½	Sultry
3	59	64	62	29.5	Do.
4	61	62	61	29.5	Rain
5	60	60	62	29.3	Sunshine
6	60	62	62	29.1	Do.
7	58	60	62	29.2½	Do.
8	57	60	60	29.6	Cool
9	57	58	58	29.5½	Foggy
10	55	61	60	29.5½	Do.
11	58	60	60	29.4	Do.
12	58	60	60	29.3	Windy
13	58	60	60	29.4	Sultry
14	57	63	60	29.3½	Cool
15	58	64	64	29.4	Do.
16	59	59	59	29.5	Rain
17	60	61	60	29.5	Windy
18	60	62	61	29.2	Do.
19	60	62	61	29.1	Clear
20	59	60	58	29.0½	Rain
21	55	68	55	29.3½	Windy
22	53	54	55	29.6	Cold
23	54	55	54	29.5	Windy
24	55	57	57	29.5	Rain
25	55	57	55	29.0½	Do.
26	53	55	55	29.2	Sunshine
27	51	53	55	29.4	Windy
28	50	53	55	29.5	Foggy
29	54	55	55	29.4	Rain
30	55	57	55	29.5	Sultry

October, 1808.	Thermom. Morning	Noon	Evening	Barometer. Degrees, A. M.	State of the Weather, A. M.
1	57	59	57	29.4	Sultry
2	58	57	58	29.4	Rain
3	54	55	56	29.5	Clear
4	54	55	55	29.5	Cool
5	54	55	55	29.3	Foggy
6	54	55	54	29.2½	Windy
7	52	53	54	29.3	Cool
8	51	54	54	29.6	Foggy
9	55	56	56	29.5	Windy
10	55	57	57	29.2	Sultry
11	55	57	56	29.3	Foggy
12	56	57	57	29.4	Rain
13	55	57	58	29.4	Windy
14	54	56	57	29.5	Cool
15	56	57	56	29.4	Warm
16	54	55	54	29.3	Sultry
17	54	55	55	29.2	Rain
18	54	52	53	29.5	Do.
19	52	53	53	29.2	Frost
20	52	53	51	29.6	Rain
21	56	55	56	29.6	Sultry
22	56	57	56	29.4	Rain
23	55	56	57	29.3	Windy
24	54	55	54	29.6	Cold
25	52	53	54	29.6	Cool
26	54	55	54	29.4	Foggy
27	55	55	55	29.5	Cool
28	55	56	56	29.4	Windy
29	52	54	55	29.7	Frosty
30	53	54	55	29.7	Clear
31	51	58	59	29.8	Frosty

November, 1808.	Thermom. Morning	Noon	Evening	Barometer. Degrees, A. M.	State of the Weather, A. M.
1	52	53	54	29.8	Foggy
2	53	54	55	29.7	Frosty
3	54	55	54	29.6	Sultry
4	55	56	57	29.5	Foggy
5	56	57	57	29.5	Do.
6	57	56	55	29.3	Windy
7	53	54	55	29.5	Cold
8	54	55	55	29.4	Cool
9	53	54	55	29.2	Sultry
10	54	56	56	29.2	Foggy
11	55	56	56	29.2	Warm
12	54	55	57	28.8	Do.
13	58	58	58	28.7	Rain
14	49	50	54	28.9	Cold
15	50	57	58	29.1	Snow
16	58	57	58	29.9	Windy
17	49	50	50	29.2	Do.
18	50	53	55	29.3	Cold
19	53	56	55	29.5	Frost
20	55	56	57	29.5	Sultry
21	53	54	55	29.5	Do.
22	56	56	55	29.2½	Do.
23	54	55	54	29.3	Warm
24	53	54	55	29.4	Windy
25	52	54	54	29.4	Do.
26	49	50	52	29.4	Do.
27	50	55	55	29.4	Calm
28	55	55	55	29.4	Hail
29	51	56	55	29.5	Cool
30	55	56	55	29.3	Do.

December, 1808.	Thermom. Morning	Noon	Evening	Barometer. Degrees, A. M.	State of the Weather, A. M.
1	56	56	56	29.4	Sultry
2	57	56	56	29.5	Rain
3	55	56	55	29.1	Do.
4	55	55	54	29.3½	Do.
5	50	50	52	29.5	Windy
6	55	55	55	29.5	Do.
7	54	55	55	29.6	Snow
8	53	54	56	29.9	Do.
9	47	56	56	29.7	Frosty

DISEASES.	Under 2 years.	From 2 to 5.	From 5 to 10.	From 10 to 20.	From 20 to 30.	From 30 to 40.	From 40 to 50.	From 50 to 60.	From 60 to 70.	From 70 to 80.	From 80 to 90.	From 90 to 100	From 100 to 110	Ages unknown.	Total.
Amenorrhœa	0	0	0	1	0	0	0	0	0	0	0	0	0	0	1
Abortion	0	0	0	0	1	0	0	0	0	0	0	0	0	0	1
Bilious remittent fever	0	0	0	0	1	0	0	0	0	0	0	0	0	0	1
Convulsions	5	0	0	0	0	0	0	0	0	0	0	0	0	0	5
Consumption	0	0	0	0	2	0	0	1	0	0	0	0	0	0	3
Cholera	2	3	0	0	0	0	0	0	0	0	0	0	0	0	5
Cynanche tonsillaris	0	1	0	0	0	0	0	0	0	0	0	0	0	0	1
Dropsy	0	0	0	0	1	0	0	0	0	0	0	0	0	0	1
Drunkenness	0	0	0	0	1	1	0	0	0	0	0	0	0	0	2
Dyspepsia	0	0	0	0	0	0	1	0	0	0	0	0	0	0	1
Hives or croup	3	0	0	0	0	0	0	0	0	0	0	0	0	0	3
Hysteria	0	0	0	0	1	0	0	0	0	0	0	0	0	0	1
Infant. remittent fever	0	1	0	0	0	0	0	0	0	0	0	0	0	0	1
Locked jaw	1	0	0	0	0	0	0	0	0	0	0	0	0	0	1
Pleurisy	0	0	0	0	0	0	0	1	0	0	0	0	0	0	1
Schirrus in utero	0	0	0	0	0	0	0	1	0	0	0	0	0	0	1
Teething	1	0	0	0	0	0	0	0	0	0	0	0	0	0	1
	12	5	0	1	7	1	1	3	0	0	0	0	0	0	30

Deaths in each month of the foregoing period.

	Adults.	Children.	Total.
January	2	2	4
February	1	0	1
March	1	3	4
April	0	2	2
May	2	2	4
June	1	1	2
July	1	3	4
August	1	0	1
September	2	1	3
October	2	1	3
November	0	1	1
December	1	0	1
	14	16	30

The population of the borough of Harrisburgh, is estimated at 2,500 souls and upwards.

The foregoing statement is drawn up with as much accuracy as possible, from the returns given to me from the sextons of both the English and German burying-grounds, and likewise from my own practice.

Harrisburgh, Dec. 12, 1808.

Omission. In the title of the Observations on Absorption, p. 55, insert By John Redman Coxe, M. D.

MEDICAL MUSEUM.

VOL. VI.....No. II.

Observations on the Yellow Fever. By JOHN STEVENS, *Esq. of Hoboken, New Jersey.*

(Continued from page 10.)

THE contagious or non-contagious (or, rather, the infectious or non-infectious) nature of pestilential diseases has ever been a subject of controversy among physicians and philosophers. This diversity of opinion has arisen, not merely from a want of accurate observations, but very much from a vague and indefinite use of terms. Had the terms in use among medical men in their disputes on this subject been precisely and accurately defined, the controversy would probably have been determined long ago.

The term contagion has scarcely been used by any two authors in the same sense. The moderns, however, have given a latitude to the term which is not warranted either by its etymology or by the authority of the ancients. This term, in conformity to its derivation, was formerly restricted in its application to those diseases only which are communicable by immediate contact, such as itch, syphilis, leprosy, hydrophobia, &c. This limitation of the term, to preserve precision and and propriety in our discussions, should be again restored;

and to all diseases communicated by or through the atmosphere, the term infection should be applied.

Again, all infectious diseases arrange themselves under subdivisions resulting from characteristic distinctions in the nature of the diseases themselves. Thus, to those diseases which are communicated by the effluvia of secreted matter emanating from a diseased person, and without undergoing any change in passing through the atmosphere, the term specific infection ought to be exclusively appropriated. As those diseases which are to be ranked under this division are, with very few exceptions, not communicable a second time from one person to another, the arrangement is in strict conformity to nature.

Again, where a disease is contracted from some noxious quality in the air itself, it may, with strict propriety, be termed atmospheric infection.

Let me here indulge myself in a remark evincive of the beauty of this natural arrangement. Those diseases which are the product of contact merely, such as itch, leprosy, syphilis, &c. are in general loathsome, and seem to be designed by an all-wise Providence as correctives against moral and physical depravity.

To diseases which are specifically infectious, many of them being extremely mortal, mankind are not in general liable to a second attack, as otherwise the world might be totally depopulated. Again, diseases originating from noxious qualities in the atmosphere itself, are for the most part, by due vigilance and attention, under the controul of man.

The arrangement here proposed obviates completely all difficulties and uncertainty respecting contagious and infectious diseases. If a disease is communicable by contact only, it is evidently a contagious disease. If it is communicable through the air, it is an infectious disease. If, at all seasons and in all situations, it is communicable immediately from one person to another, through the air, it is specifically infectious, especially if not to be taken a second time. There are, however, some specifically infectious diseases, which are also communicable by contact; for instance, small-pox. These diseases, then, are so far both contagious and infectious. But if, for the communication of a disease through the air, some noxious quality in that air is necessary, it then becomes an atmospheric infection. How easy is it, then, to determine in most cases whether a disease is specifically or atmospherically infectious. Let us take an instance of each species: small-pox and yellow fever. As the small-pox is communicable, through the air, from one person to another, at all seasons, and in all situations, and no person liable to a second attack of it, it is, unquestionably, a specific infection; but, on the other hand, as yellow fever is the effect of some noxious quality in the atmosphere itself, prevails only at certain seasons and in certain places, and as a person is liable to repeated attacks of it, can we hesitate a moment to pronounce it an atmospheric infection?

Atmospheric infections require the concurrence of certain seasons to render them epidemic. Universal experience has confirmed this fact. Thus, in Egypt, the plague commences in the latter part of the winter, and ceases with so much regularity that the natives are apprehensive of no danger after the 17th day of June. In Syria, also, its visitations are governed by similar periodical regularity. I need scarcely observe that

with us yellow fever has observed a like uniformity with respect to its appearance and disappearance.

This circumstance furnishes a characteristic distinction between specific infections and atmospheric infections. In a disease of the former description, the process on which its infecting quality depends is wholly completed in the body of the diseased person. Now, as it is well known, that the state of the fluids of the body, as well as its temperature, continue nearly the same at all seasons of the year, so we find that specific infections have scarcely any dependence on the vicissitudes of seasons and climates ; whereas, on the other hand, in all atmospheric infections, the process on which their infecting quality depends is carried on in the atmosphere itself. Accordingly, therefore, to their respective natures, they require particular temperatures and seasons to bring them to maturity.

The ordinary process of putrefaction, whether of animal or vegetable substances, will not produce atmospheric infections. There is an inexplicable something, besides mere putrefaction, necessary to give a morbid quality to the atmosphere. The putrefaction of a dead horse, for instance, will annoy a whole neighbourhood with its stench, but generally no other inconvenience or mischief attends it. Thus, too, the farmer is under no apprehensions of danger from the putrefaction of animal and vegetable substances in his dung-heap, perhaps at his very door.

Dr. Mitchill* mentions a very remarkable fact corroborative of this doctrine. "Seven men," he observes, "belonging to the alms-house of New York, were employed, during the whole of the sickly season of 1798, in putting the persons dead of the plague (yellow fever) into coffins; and though they handled,

* Medical Repository, Vol. II, page 313.

in the course of their service, upwards of five hundred corpses, in different stages of putrefaction, and though they were much incommoded with the pestilential quality of the air in the rooms they entered, and frequently were obliged to vomit, not one of them was so much indisposed, during the whole of the season, as to discontinue his employment."

The celebrated Howard, speaking of the infectious nature of the plague, says, that "it is remarkable that, when the corpse is cold of a person dead of the plague, it does not infect the air by any noxious exhalations. This is so much believed in Turkey, that the people there are not afraid to handle such corpses. The governor of the French hospital at Smyrna told me, that in the last dreadful plague there, his house was rendered almost intolerable by an offensive scent; especially if he opened any of those windows which looked towards the great burying-ground, where numbers every day were left unburied; but that it had no effect on the health of himself or family."

These, and a thousand other facts of daily occurrence, prove, incontestibly, that the ordinary products of putrefactive processes are not alone the causes of atmospheric infections, but that some further process is required, which, of course, varies according to the nature of the disease thereby communicated.

Dr. Rush, in attributing yellow fever, in 1793, to putrid coffee, and Dr. Mitchill, in 1798, to putrid beef, were certainly incorrect; for, if those causes alone were sufficient to produce yellow fever, the disease would manifest itself every season, invariably, in various parts of the country: but this never happens. Out of sea-port towns yellow fever is nowhere to be found.

Some of the supporters of the domestic origin of the yellow fever, who have taken great pains too to prove that yellow fever is actually generated in the atmosphere of our cities, have, however, very inconsistently urged, that when yellow fever rages with peculiar malignancy, and becomes epidemic, that it becomes also specifically infectious. But when it is ascertained beyond all doubt that the atmosphere of a place will give the disease without any communication with the diseased, what evidence can we possibly have of any case of specific infection? If then, from the nature of the case, it is utterly impossible to adduce proof of specific infection in any one instance, it is surely inconsistent with reason and sound philosophy to have recourse to it, when every case that can possibly happen may be satisfactorily accounted for on the principles of atmospheric infection; and when we know, too, that the former has no existence whatever out of the sphere of the latter. The chain of reasoning here adduced propels us irresistibly to this conclusion: that yellow fever is never the result of specific infection, but is invariably the product of noxious combinations taking place in the atmosphere.

A cloud of evidence might, indeed, be adduced to prove that yellow fever is not specifically infectious. The exemption of physicians, nurses, washerwomen, and attendants, even in hospitals, besides the vast number of cases of individuals sickening and dying in the country without communicating infection, has placed this fact beyond the reach of controversy.

It will unquestionably be found, on investigation, that no malignant fever which evidently originates from noxious combinations taking place in the atmosphere itself, and therefore becomes epidemic, is, in any circumstance, the offspring of specific infection. Even the plague, which we have hitherto

been taught to believe so specifically infectious, is now proved to be not in the least so. Colonel Wilson, who has lately published a history of the British expedition in Egypt, and who, attached to the army, was himself on the spot, tells us expressly, "that the English and Turkish armies which marched to Cairo, passed through a country where the plague filled almost every village; they communicated, without any precaution, in the most intimate manner with the natives, established their ovens at Menouf, where the plague raged violently: the Turks even riffled the diseased in the pest-houses at Rhamanich, and at Cairo dug up the corpses recently buried, and yet no individual instance occurred of the malady in the armies."

In proof that jail fever also is in nowise specifically infectious, I will adduce two very remarkable cases.

First, The memorable catastrophe which happened at what are called the black assizes at Oxford, in the reign of queen Elizabeth. Camden's account of this affair is as follows. "While the judges of assizes sat at Oxford, and one Rowland Jenkes, a saucy foul-mouthed bookseller, was indicted for scandalous words against his princess, the greatest part of those who were there present, whether through a poisonous and pestilent vapour, or the stink of the prisoners, or damp of the ground, were taken in such a manner, that they died almost every one of them, within forty days, or thereabouts, except the women and children; and *none else were touched with the contagion.* Amongst those that died were Robert Bell, lord-chief baron of the exchequer, Sir Robert D'Oiley, and Sir William Babington, knights, D'Oiley, sheriff of Oxfordshire, Harcourt, Wineman, Fettiplace, men of great note in those parts; Barham, an excellent lawyer, almost all the jury (as they call 'em), and others to the number of 300, or

thereabouts." Stowe also tells us, that of this fever " died in Oxford 300 persons ; and sickened there, but died elsewhere, more than 200 from the 6th to the 12th of July. After which died not one of that sickness, *for one of them infected not another*."

The other is a similar case, which I here give in the words of Mr. Webster. " So also at the Old Bailey, not many years past, a few prisoners entering the court from a dirty jail, without changing their dress, infected a large number of persons who died ; but no epidemic followed : the infection was soon dissipated, and there was an end of the disease."

These cases do not furnish ground even for a surmise that any disease was communicated from the prisoners themselves. And in neither case was the disease communicated, in a single instance, from those who caught it in court, to any other person.

The disease, in these cases, originated altogether from the noxious atmosphere with which the bodies of those prisoners were enveloped, and in nowise from individual infection : it is with the utmost precision and propriety, then, that the term atmosperic infection is applied to it.

That jail fever is not specifically infectious, the exemption of the nurses and attendants at the Marine Hospital affords ample testimony. Notwithstanding the vast numbers of emigrants who have, from time to time, been received into the wards of this hospital, yet have these nurses and attendants generally escaped sickness. Were this disease, indeed, individually infectious, in crowded ships and prisons, there would be

scarcely a possibility of escaping when the disease was prevalent.

That this disease, therefore, like yellow fever, plague, &c. is the result of noxious combinations formed in the atmosphere, and never from secreted matter emanating from a diseased person, is sufficiently evinced from the circumstances of the two cases above cited; but it differs from yellow fever, &c. in these respects: that it prevails chiefly in the winter season, and that the atmosphere in which it is generated is circumscribed within narrow bounds. I am inclined to think that the plagues which from time to time have formerly infested Europe during the winter season, have been of a nature similar to jail fever. The disease has lately raged in some of the manufacturing towns in England, with a degree of virulence and mortality not much inferior, within its sphere of action, to that of yellow fever in this country, during its worst stages.

As the gaseous compounds, whence atmospheric infections derive their origin, are capable of infinite diversification, so we may rationally presume, that the diseases they give rise to must also assume various types, analogous to the nature of these compounds. As plague, yellow fever, jail fever, &c. are always generated in cities or in crowded places, it is presumable that animal exhalations are the predominant ingredients of the combinations from whence they originate; and, on the other hand, that bilious fever, remitting fever, and intermitting fevers result principally from combinations framed from vegetable exhalations. Dysentery appears to be occasionally the result of either animal or vegetable exhalations, but assumes more or less malignancy in proportion to the predominancy of the animal ingredients. In camps, the infection is principally derived from excrementitious matters. Of all this numerous

family of atmospherically infectious diseases, none has been considered so specifically infectious as dysentery. But as no disease is more evidently the product of animal and vegetable exhalations, as it appears only in certain seasons and in certain places, and attacks the same person repeatedly, it is presumable we may, upon analogical principles, safely pronounce it at no time specifically infectious.

Should my theory prove to be well founded, the circumstances attending the propagation of dysentery will throw considerable light on the manner in which yellow fever also is propagated. It is an undoubted fact, that the excretions and excrementitious matters from a person labouring under this disease become highly infectious if not carefully removed. For this reason it is, that dysentery rages with such violence within an encampment; so that, in a short time, the atmosphere becomes extremely infectious.

In this particular the analogy between dysentery and yellow fever is very striking; for, in the latter disease, the remarkable case of Mrs. Baker, which will be mentioned presently, furnishes unequivocal proof of excretory matter in like manner becoming infectious; and there cannot, I think, be a shadow of doubt, but that, in favourable circumstances, a ferment of this nature would, in this disease, as in dysentery, infect a certain portion of the atmosphere. The infection of the plague is, beyond all doubt, conveyed from place to place by similar ferments; and the two instances already cited of prisoners in health communicating jail fever to numbers in the court, are direct and positive proofs that infection in this disease is also capable of being conveyed from place to place by *clothing;* and they are also striking instances in proof of this disease not being in any degree specifically infectious.

Should the theory I have advanced respecting diseases atmospherically infectious prove, on further investigation and experience, to be well founded, it will point out to us precisely where danger is to be apprehended; we shall avoid being taken by surprise, and being panic-stricken by false alarms. When we shall become thoroughly satisfied that a disease generated in the atmosphere can never, in any case, be communicated immediately from one person to another, but that it is only from excretory matter that danger is to be apprehended, we shall be taught where to guard against danger, and to perform our duties towards the sick with confidence and alacrity.

(To be continued.)

The Doctrine of Pulmonary Absorption defended. Communicated to the Editor by JOSEPH KLAPP, *M. D.*

IT is somewhat surprising that persons should be found to disbelieve absorption by the lungs, as it is so generally known that these organs are abundantly supplied with absorbing vessels, arising with large and patulous mouths from the air cells. If spirit of turpentine, mercury, and many other substances, be conveyed into the general circulation by the absorbents of the stomach and intestines, why may they not also be taken in when applied as fairly to the mouths of the lymphatics in the lungs, as can be the case in those organs, or any other? The existence of cuticular aborbents I must be permitted to regard as a mere hypothesis; and, being imaginary, no man can be authorized to demand a description of *their use*. But such

vessels have often been demonstrated to exist in the lungs, and if pulmonary absorption is to be denied, " as it is clear so large a congeries of vessels could never be proposed for no use, I think *we are warranted* in demanding what purpose they are intended to answer?"

In order to show that the lungs are abundantly supplied with absorbing vessels, I shall preface my experiments to prove that they do perform the office of absorption, by a few quotations from Mr. Cruikshank's work on the lymphatic system.

" Next to the liver, the lungs are that part in which I have found the greatest number of absorbent vessels." Vide page 175.

" The absorbents of the lungs are also divided into two sets, a superficial and deep seated. The superficial set, like those of the other viscera, are not always to be found. I have been able, at one time, to cover the whole external surface of the lungs with the absorbents I had injected; and at another time I have not been able to find one. The same thing I have known happen frequently in the liver.

" One of the easiest methods of finding them, is to inflate the lungs of a still-born child from the trachea; *the air passes instantly from the air cells*, and fills particularly the superficial absorbents; if a puncture is then made with a lancet into any one of those absorbents, the air will escape, and quicksilver may then be injected in its place, as the valves hinder it from escaping into the air cells. One of the most perfect preparations I ever made of the absorbents of the lungs was in this way." Vide page 176.

The advocates of non-cuticular absorption never intended to be understood that they believed the lungs " the exclusive apparatus of absorption." I always supposed they denied absorption from the surface only, and did not intend that their experiments and arguments to prove pulmonic absorption should be deemed by any one as an attempt to disprove its occurrence in the stomach, intestines, and in other parts. At least I can say, with regard to myself, such has always been my intention or meaning. Surely there could be no inconsistency in denying absorption from the surface, and at the same time *even contending* that it is executed by the pulmonary organs, as well as by all those parts in which it has been fully proved to exist*.

It is possible that, in stating the discovery of pulmonary absorption, its supporters may have spoken of it in rather an emphatic manner; but I am certain they thereby never thought of denouncing absorption from many other parts of the animal body. I trust the candid reader, and the most of writers, will unite in asserting, when the following experiments and other considerations are with deference submitted for their deliberation, that absorption by the lungs is not quite so vague, or hypothetical, as some have represented it. In the first place I shall take the liberty of quoting an experiment from my inaugural essay, to show that it is not quite true, as has been stated, that " in *every case* when the violet smell was perceptible in the urine, the turpentine was carried both into the stomach and lungs."

* However this may have been the intention of the author of the present essay, it certainly was not the case with some of the most strenuous opponents of cuticular absorption. EDITOR.

" A strong dog being fastened down upon a table, an incision, as in the former experiment, was made just above the superior extremity of the sternum, and at this place a ligature was passed around the trachea, which completely prevented the animal from respiring through his mouth. A small aperture was then made below the ligature into the wind-pipe, to which was adapted one end of a long tube, while the other end communicated with the mouth of a bottle containing spirits of turpentine. In this situation the dog continued to inhale by his lungs emanations of turpentine for two hours. The operation was then discontinued, the tube was removed together with the ligature about the trachea, and the incisions which had been made were closed: the mouth, fauces, and trachea of course resumed their former functions.

" The dog was now left from eleven o'clock, A. M. to half after six in the evening, when he was killed. The urine collected (a ligature having been previously passed around the penis for that purpose) gave a strong smell of violets. My obliging friend, Dr. Thomas Smith, and myself, made ourselves acquainted with this circumstance by repeated examinations." In addition to what is mentioned, it may be proper to state, that very particular means were used to prevent the animal from inhaling the turpentine by the mouth, both before and after the experiment. The turpentine was carried into the room in a bottle closely stopped; and, previous to the removal of the tube and ligature from the trachea, the dog was carried into another part of the house, where the smell of the turpentine was not perceivable, and in which place he continued until he was killed. This experiment, which was certainly conducted with all necessary fairness and precision, I must insist, very clearly proves absorption by the lungs, and that too under precautions which must have precluded " any portion of the

turpentine from having passed into the stomach entangled with the saliva." It in all probability has not been noticed, " in referring to the experiments detailed in the essays," or it would not have been asserted, " that the gentlemen cannot urge an objection against the one, which is not equally conclusive against the other, nor advance a fact which will not be as favourable for my opinion as for theirs."

Although I believe it will be allowed that the above experiment is very conclusive, at least with regard to the animal it was made on, yet the next one may be more satisfactory, on account of its having been performed on the human body. And as it can be repeated at any time without trouble, I hope every reader will do so for his own satisfaction.

On the 4th of April, 1809, having cleared my mouth and throat of saliva by spitting several times, I began to breathe the emanations of the turpentine out of a half gallon tincture bottle, during four deep inspirations, using the precaution not to swallow the saliva, after which I returned the stopper into the bottle, and again spit out all the saliva that had collected while inhaling the turpentine. After a minute or two had elapsed, I walked into the part of the room where the bottle was standing, and, on raising the glass stopper, I again inspired the turpentine an equal number of times, taking care to abstain from swallowing, and afterwards discharging the saliva by frequent spitting. This process I carried on for upwards of twenty minutes, alternately inhaling the emanations of the turpentine, and cleansing my mouth without having in any one instance swallowed my saliva. Before the time just mentioned had expired, my head began to ache, and I felt an unpleasant sensation in my breast. When three quarters of an hour were elapsed, the urine was inspected, and I think I was never sen-

sible of the violet smell in it to so great a degree. It was afterwards frequently examined; I think as often as five or six times, and on every trial the peculiar smell was more or less perceptible.

The reasoning made use of in the last number of the Museum, to explain why spirit of turpentine cannot be absorbed through the skin, though I perfectly concur with the writer that it cannot, I must be permitted to consider as unsatisfactory, since it would lead to a conclusion which is contrary to fact, viz. it would lead us also to deny its absorption from every part of the animal body. It has long been a very common, and, so far as I know, a very just saying, "it must be a poor rule which will not work both ways." If the supposed "numerous lymphatics which are spread over the surface of the body" are too fastidious to take up spirits of turpentine, it would be improbable they would be less high-minded in the stomach and lungs. Yet in those organs these dainty gentlemen do condescend to partake of "so acrid and stimulating," and consequently so unsavory a dish as spirits of turpentine. The lymphatic system, like other subordinate ones, has its own laws, and I trust these will not be found to contradict each other, or be essentially different in any of its parts. I do not believe that there are any absorbents which commence with open mouths from the surface of the cuticle. The cellular membrane is doubtless an extensive bed *for*, and may be it would be just to say *of* them; and they may originate from the cutis vera, agreeably to Mr. Cruikshank's opinion. They appear to be covered and protected from the noxious agency of all external causes, by a casement, or "a seven-fold shield," formed of an insensible, and I think, though it is offered as a mere matter of opinion, inorganic cuticle: of course it would be impossible for them to take up substances applied to the

sound surface, with which, from the interposition of the epidermis and rete mucosum, they could not come in contact. Situated as they are, they no doubt perform very important services in the animal economy, of which the occasional conveyance of fat from the cellular membrane into the general circulation, and of fluids from the cutis vera, which appears to be the organ of perspiration, perhaps constitute no inconsiderable part of their duty; and, when we advert to the admirable provision the Author of Nature has made for the protection of vessels so essential to the carrying on the important labours of the living machine, we must all feel sensible of an impulse to renew the truly contemplative and pious remark, "Fearfully, indeed, and wonderfully are we made, when such various channels of preservation from destruction so clearly point out the superlative wisdom of a Divine Architect."

With regard to mercurial ointment, the ingenious writer, in the last Museum, seems to have had experience of its absorption through the skin; which, if true, it must be confessed, would be unusual, and prove very readily the truth of the Galenic theory, which he advocates. This experience seems to be no less than "the evidence of his senses." Seeing, it has been said, is believing. I suspect there are not many writers to be found, who have deemed the impossibility of detecting mercury in the fluids of the body, as a conclusive argument against its absorption through the skin. This circumstance, as has justly been remarked, "can only be regarded as negative, not positive proof." "I must be allowed," he says, "to deny my faith to the extraordinary manner by which they suppose it to be introduced into the system, viz. by being carried in a state of volatilization into the lungs." And in the next passage but one, he says, "I certainly may be allowed to demand of them their proof of this assumed fact."

The following considerations, founded on experiments, will, I suspect, incline the most of readers not to conclude that the inhalation of mercurial vapours by the lungs is "either extraordinary, or an assumed fact." If mercurial frictions produce a salivation by absorption through the skin, then mercurial ointment, applied to it in such a way as to intercept all communication between it and the lungs by respiration, ought to excite a ptyalism just as soon as if no such precaution was used. The following experiments, however, made by Dr. Daingerfield, prove that this is not the case.

"To accomplish these important ends, I selected a substance, which, from the uniformity with which it has been supposed to produce its effects through the medium of absorption, promised results highly satisfactory. The substance alluded to is mercurial ointment. To ascertain whether it did or did not produce salivation, together with its other effects, in consequence of being taken into the general circulation, I applied large plasters of it to the calves of my legs; but, as I was apprized of the objection, that the mercury might be volatilized and taken into the lungs unless means were adopted to prevent it, and that, if applied to an abraded cuticle, the experiment would be inconclusive, care was taken to avoid both the one and the other of these sources of error, by applying them to a perfectly sound cuticle, and by covering them with thick bladders, rollers, and a pair of stockings, which were not removed until the end of the experiment. Thus circumstanced, I waited patiently the arrival of a speedy salivation; but, at the end of eight days, was completely disappointed, never having observed the slightest alteration in my general health, nor in my pulse, nor in the discharge of saliva. Not content, though, with this, and willing to believe that the result of my experiment had failed to correspond with my expectations, only

in consequence of want of attention to a restricted diet, I accordingly resolved to liye exclusively on vegetables, and to eat even of these with moderation. This determination was completely carried into execution, but with no better success; for, at the end of another week, the plasters had certainly produced no sensible effect. Unwilling, though, to abandon a doctrine so generally acquiesced in, and ardently desiring to know something conclusive on this subject, I reduced my system still farther, by the loss of fifteen or twenty ounces of blood, continued my vegetable diet, and applied, with the former precautions, two large mercurial plasters to *my fore arms*, where they were suffered to remain seven or eight days; and, at the end of which time, as they had produced no effect whatever, I put an end to my experiment, after its having lasted the greater part of three weeks." "From the foregoing facts," continues my indefatigable friend, "it appeared that there was no active power of cutaneous absorption; since, if there was, I ought to have been salivated, as having lived under every circumstance essential to that event. Afraid, though, to trust this conclusion to conjecture, when it could be made the subject of direct experiment, and apprehending that my experiments might possibly have failed from want of activity on the part of the absorbents to which the plasters were applied, or from some idiosyncrasy of constitution, or from some imperfection in my mode of living, or in the quality of the ointment used, I resolved to try the effect of frictions. Accordingly, on the same parts, with much less of the same ointment than had ever been applied at any one time in the form of plasters, I succeeded, in three nights, in gently affecting my mouth." The doctor goes on to observe, "from the foregoing experiments it would appear, that the absorbents stimulated by the friction used in the application of the ointment, were compelled to take it into the circulation. To determine, therefore,

how far this was really the case, it was necessary that the ointment should be applied to parts, which, at the same time that they possessed the advantages of friction, should leave nothing to apprehend from its being volatilized and taken into the lungs. Accordingly, I provided a strong pair of oiled-cloth socks, applied an ounce of unguentum hydrargyri fortius to the upper surface of each of my feet, put on the socks, drew a pair of stockings over them, and regularly walked a mile or two every day, that the friction of my boots against my feet might cause the mercury to be absorbed. At the end of ten days, I was greatly astonished to find that they had produced no effect. The socks were therefore taken off, and the same quantity of ointment again applied; after which they were renewed and permitted to remain sixteen or eighteen days longer; but, as in this time no alteration had taken place in the state of my salivary glands, they were again removed, and the experiment considered as concluded. It may not be improper to add, that my diet on this occasion was low, and strictly vegetable.

I trust it will be granted that mercurial frictions do produce a salivation in either the one or the other of the three ways by which their operation has been explained by different persons. 1. By absorption. 2. By sympathy. 3. By the inhalation of the mercurial vapours. Now, the establishment of any one of these contending theories must follow a refutation of the two others. I shall therefore proceed on the subject with this mode of reasoning. It will hardly be denied that the ingenious experiments of Dr. Daingerfield, which I have quoted at large, prove very clearly, that mercurial ointment is not absorbed through the skin; the question then recurs, how does the mercury pass into the system to occasion a salivation? I say pass into the system, because without its doing so, I be-

lieve a salivation cannot be produced. The doctrine of sympathy will not do, since the experiments related are equally obnoxious to it as to the hypothesis of cuticular absorption. The ointment was confined on the legs and arms the greater part of three weeks, without occasioning a salivation either by absorption or the sympathy of the nerves. I am well aware, in entertaining this opinion, I differ with Dr. Daingerfield, who has accounted for the operation of the mercurial ointment, by the doctrine of a sympathetic connection between the skin and the salivary glands: but the evidence of his own experiments related above, is so strong and so pointed in favour of what is termed " the volatile theory," that I am not prepared to agree either with the doctrine of absorption, or his of sympathy. The distance between the calves of the legs, or even that of the feet and ankles, and the throat, does not, according to my way of thinking, afford a reasonable explanation why the ointment did not salivate. Besides, " large mercurial plasters were also applied *to his fore arms*, where they were suffered to remain *seven or eight days*, and at the end of which time, as they produced *no effect*, he put an end to the experiment," &c. I do not hesitate to affirm, and I believe an impartial experiment would support me in the assertion, that if mercurial frictions on the feet and calves of the legs are persevered in a much less period of time than " ten days, or the greater part of three weeks," allowing a free access to the lungs, a salivation can be excited. In infancy, the policy or the government of the nervous system may not be sufficiently organized or accustomed to impressions, to establish at so early a period of animal life its different associated or " catenated motions," to use a Darwinian expression, and hence, perhaps, it may be correct to say, " children under a certain age cannot be salivated, because those two sets of vessels, the stomach and salivary glands, have not acted long enough together for their motions to become associ-

ated." But at the mature period of *adult life*, when it is presumable the various *customary* movements of the nervous system must have acquired a strong tendency to *habits*, it would not do to apply that doctine. Where there is no analogy, the analogical mode of reasoning will not answer, and it must be admitted, with regard to the associations of the nervous system, in "children under a certain age," and in adult life, the analogy is very slight.

It is thus I would oppose the doctrines of cuticular absorption and of sympathy, and deduce, as they are inadequate to explain the interesting fact in question, that the salivation must arise out of the evaporation of the mercury from the skin, and its consequent inhalation by the lungs. If a salivation cannot be produced by rubbing mercurial ointment on the skin, when the lungs are insulated, as Dr. Daingerfield's experiments prove, and as another experiment also does, which will be presently related, I ask, what is the conclusion that follows? Why, clearly, that the failure in producing that effect was owing to the mercurial vapours having been excluded from the lungs. But, in obeisance to the precept of the justly celebrated Bacon, "fiat experimentum," and, to use the language of my ingenious fellow-graduate, "afraid, though, to trust this conclusion to conjecture, when it could be made the subject of direct experiment," I shall proceed to detail what I humbly trust is a confirmation of it. The experiments were made about a twelve-month since. I shall extract them verbatim from an unpublished work on the subject of cuticular absorption. "In the first place, I ascertained that my mercurial ointment was of an active kind, and that in my constitution there existed neither temperament nor temporary irritation to oppose the production of a salivation. By one hour's rubbing of the ointment on the skin, twice a day, for three days together, a ptyalism

was produced. The present experiment was then commenced, by causing my boy, a young man, about fifteen years of age, to rub, briskly, the ung. hydr. fort. on the inside of my legs, an hour at a time, and twice a day. During each time the frictions were applied, every necessary precaution was used to exclude the mercurial vapours from the lungs, by breathing, in a long tube placed through the window, the pure air of out doors. As my system at this time was to all visible appearances in as susceptible a state to the operation of mercury as when a salivation was first excited, if the doctrine of cuticular absorption, or that of sympathy, be true, it would be reasonable to anticipate a salivation in three days from the beginning of the experiment. The frictions, however, were unremittingly continued to the sixth day; and, though no inconsiderable quantity of the strongest mercurial ointment was wasted at each time the frictions were used, yet nothing like a salivation was thereby produced. No tenderness or soreness of the mouth, no increased secretion of saliva, and no fœtor of the breath, ensued. At first, it was my intention to continue the experiment much longer; but the condition of my assistant on the sixth day, whose case I shall shortly describe, was such as to render a further continuation of the frictions improper. The more effectually to guard against the inhalation of the mercury by the lungs, whenever each hour of the frictions was completed, I constantly withdrew to another part of the house, and there had the ointment washed from the skin with soap and water; after which I again dressed myself, and usually renewed my attention to those professional duties which had a claim on me as a practitioner of medicine."

The next experiment is as follows. "I have mentioned that my assistant was so much indisposed on the sixth day of the last experiment, as to render a further continuation of it

improper. This case, which will constitute the present experiment, exhibited a set of symptoms very familiar to the physician. An evident tumefaction of the submaxillary glands, a soreness of the mouth, and a pretty copious excretion of saliva, made up the outlines of his indisposition. In short, he was completely salivated, though, from the very beginning of the frictions, precautions were used to prevent the mercurial ointment from coming in contact with his skin. He wore a very tight glove on the hand with which he applied the mercurial ointment, and the mouth of which was laced closely around the wrist. I acknowledge that the situation of my young man was much more favourable to the inhalation of the mercurial vapours than is usually the case with patients on whom the frictions are used; but the difference could not be so great as to admit of absorption by the lungs *only in his case*, and not in common ones. He usually sat down on the floor while he was rubbing the ointment on my skin, and the posture or inclination of his body was such, that, at every inspiration he made, the mercurial vapours were very plentifully applied to his lungs. On the sixth day of the experiment, as has been mentioned, he was salivated, notwithstanding the mercurial ointment did not come in contact with his skin."

These experiments, and those of Dr. Daingerfield, appear to establish, in a very satisfactory manner, the theory I have embraced, of the manner mercurial ointment rubbed on the skin produces a salivation. I think I am now sufficiently authorized to assert, that frictions of mercury do not occasion a salivation by absorption through the skin; because, when the ointment is applied separately to the external surface, though frictions of it be continued a period of time greatly exceeding that usually required to occasion a salivation, yet such an effect cannot be thereby produced. And I further conclude,

that the salivation is occasioned by the absorption of the mercury through the lungs; because, when the emanations of ung. hydr. fort. are copiously inhaled by them, a salivation ensues. Independent of the cogent reasons which have been deduced from the firm basis of experiments, in favour of the inhalation of mercury by the lungs, there is one more which at present occurs to me. It is a fact, that persons have been salivated merely by breathing for a few minutes the air of venereal wards in different hospitals which contained patients under the use of mercurial frictions. But if it were true that the heat of the body was insufficient to volatilize the mercury in the ointment, then such an occurrence could never have taken place. But it is said, " by exposure of ℥i of strong mercurial ointment, for two hours and a half, to a temperature of 120°, a degree of heat, *I* apprehend, seldom exceeded by the friction of the hand on the skin, I found it lost only fifteen grains, arising, *most probably*, from a loss of the sebaceous matter, and not of the metal." I must be permitted to assert, that, in my estimation, this experiment is entitled to no weight in deciding the question. Were the circumstances under which the ointment was exposed as favourable to its evaporation as frictions of it on the skin, the experiment might then be somewhat conclusive; but every one, at the first glance, must perceive that this could not have been the case. The particulars of the process are not mentioned; but, it is presumable, the ointment was exposed, in a cup, to 120° of heat, and the first effect which ensued must have been the liquifaction or melting of the lard. The divided or triturated mercury, on account of its gravity, would, in a short time, settle to the bottom of the vessel, and the supernatant lard, in the form of oil, would effectually prevent its evaporation into the surrounding air. Hence the small quantity of the ointment lost during the two hours and a half is not to be wondered at; and hence,

the supposition, that this loss consisted " of the sebaceous matter, and not of the metal," is not improbable. When mercurial ointment is briskly rubbed over the skin, its evaporation is greatly promoted by the extent and the thinness of its successive layers, which are equally acted on by the animal heat and by the free access of atmospheric air. But these favourable agents to evaporation, which always operate in the use of mercurial frictions, are prevented from exerting their usual influence in all such experiments as the above.

The matter of many contagious diseases, contracted in the casual way, seems to be principally conveyed into the system through the lungs. Hence, perhaps, the very considerable affection of the lungs in measles, and hence perhaps the reason the matter of small-pox will neither infect the system when applied either to the sound skin, or when taken into the stomach. I do not assert these facts from my own experience, though I do not hesitate to credit them. That of the non-absorption of the variolous infection by the skin, I believe, was first ascertained by Dr. Benjamin Rush, and the other fact originated with a physician in the state of Jersey, whose name is not at this time recollected.

The characteristic properties of the infection, it is presumable, are destroyed by the digestive powers of the stomach. The same vital or assimilating power is exerted, and the same change takes place, when the poison of the viper, and the matter of syphilis, are taken into the stomach. As is the case with the contagion of small-pox, those violent causes of disease and death are rendered harmless when taken into the stomach.

The late Dr. Currie was an advocate for the conveyance of contagion into the system through the lungs. This highly respectable author says, "if the non-absorption by the surface of the body be established, it will ascertain that, in the ordinary course of things, contagion is received into the system by the lungs only, and will justify a practice which is common among our more experienced seamen on the coast of Guinea, and other warm climates, who, when exposed during the night to a breeze from the marshes, wrap their heads in a sea-cloak, or other covering, and sleep fearless on the deck, with the rest of their bodies nearly exposed." The primary operation which the lungs and trachea sustain from the contagion of casual small-pox may be a principal cause of that noted liability of those parts to become violently and often fatally affected with this disease, at that stage when, in popular language, the pock begin to turn.

This affection of the lungs seems to partake greatly of the familiar character of the common pneumonic affections. In some cases, I think I have perceived pneumonia vera, and sometimes pneumonia notha, to succeed about the turn of the pock. If I am right in the character of this dangerous succeeding affection of casual small-pox, the propriety of using the lancet, a salivation, and other evacuating remedies for its removal will perhaps be made as reasonable *in theory* as they have heretofore been found successful *in practice*.

It may be inferred that the almost continued irritation in the lungs, so characteristic of measles, is owing to the primary operation of the infection on them, from the fact that this complaint, when produced by inoculation, is nearly or quite unattended with that mark of disease.

Dr. Wilson observes, "the chief difference between the casual and inoculated measles seemed to be the absence of any pulmonic affection at all periods of the latter."

With regard to Mr. Home's experiments, I neither am inclined nor do I conceive it necessary to say much. Not inclined, as his theory does not affect pulmonary absorption; nor necessary to say much, as this is not the place to discuss his hypothesis. His object, if I comprehend him rightly, is to establish the existence of a vascular connection between the stomach and bladder, through the spleen, unconnected with the common rout or the general circulation. It may possibly be, that a secret channel of this kind exists; but I must confess the theory seems to labour under many difficulties.

A few years since, an experiment was made in this city by Dr. Rogers, then of Lexington, and myself, to ascertain how far Darwin's theory of an occasional conveyance of fluids from the stomach into the bladder, by his supposed inverted motion of the urinary lymphatics, was well founded, which, though at that time we could not have Mr. Home's idea at all in view, as he had as yet perhaps not conceived it himself, I think will not only reflect great doubt on Darwin's doctrine, but also to an equal degree on Home's. I shall briefly relate it. An incision was made into the lower part of the abdomen of a dog, when the ureters were found, and a ligature passed around each of them within two or three inches of their entrance into the bladder. The urine contained by the bladder was then pressed out with the fingers, and, in order to retain in the bladder whatever might afterwards be deposited in it, a ligature was passed round the penis. The wound occasioned by the incision was closely stitched up, when we commenced giving every now and then a dose of gin, to produce that inebriation, which Dr. Darwin con-

ceived very conducive to invert the motion of the urinary lymphatics. After a while we gave the animal, in the gin, madder and nitre, two articles well known for producing a considerable effect on the urinary system. We dosed the devoted animal in this way for an hour or two, until we were of the opinion a sufficiency had been exhibited, when he was left about eleven hours in a room. I pithed him with a scalpel, and on opening the wound we found the ureters above the ligatures much distended with urine up to the kidneys, but not a drop of this fluid existed below, either in the ureters or bladder. This experiment induced Dr. Rogers and myself to conclude, that the ingenious author of the celebrated Zoonomia was mistaken; and as it has never been published, I now offer it to the public, not only for the single purpose it was first designed, but as an experiment showing that a direct channel through the spleen from the stomach to the bladder in all probability does not exist. Did it exist, the rubia tinctorum impregnated with nitre would have certainly made its way into the bladder in spite of the ligatures about the ureters.

I communicate the experiment and use the name of my friend without consulting him; but, from my knowledge of his love of investigation, I have no doubt of its meeting with his approbation.

Tying up the ureters previous to the introduction of coloured substances into the stomach, must appear like a much more fair as well as conclusive method of experimenting to ascertain this supposed channel, than the one used by Mr. Home. The only experiment in his communication which in the least wears the appearance of being unequivocal, is the one in which the pylorus was taken up. But, if even this be closely examined, it cannot justly retain such a conclusive character in the mind of

the scrutinizing and candid physiologist. If those direct vessels do exist, they doubtless are susceptible of having either false offices imposed on them, or their natural ones suppressed, by severe injuries; and which serious injury every one must perceive in tying up with a ligature so sensible an appendage to the stomach as the pylorus. Again, Mr. Home's experiments, granting they prove the existence of the secret passage in question, do not nor could not acquaint either himself or us with the part of the urinary system to which substances are carried. They might be discharged either in the bladder or ureters, or exclusively in the kidneys. But my experiment proves, and I must be entitled to draw a conclusion as I believe it will always succeed, that fluids are not carried from the stomach into either the lower part of the ureters, or into the bladder. The fact of substances showing themselves in the urine in the course of a short time after their introduction into the stomach, their disappearance for a while and again returning, I am ready to admit, stamps on Mr. Home's hypothesis an air of plausibility. And when I admit this, I must be permitted to caution against mistaking mere plausibility for *positive proof.* Though rather improbable, yet I believe it will be admitted that such occurrences may successively ensue through the well known and long since established channel of the general circulation; at any rate the fact stated cannot prove to conviction that a more direct channel does exist, such as Mr. Home has imagined. My promise to say not much in this place may attract observation after what I have said, but, compared with the importance of the inquiry, I in reality have said but *little.* Mr. Home has opened a new field of researches, and I believe *much* remains yet to be done. The science of medicine is yet rich with extensive fields of terra incognita, and the mere apprisal of the quarters in which they lie can only form the beginning of a series of important discoveries.

A Pathological Fact, in proof that there exists a more direct Channel of communication between the Alimentary Canal and the Bladder, than that through the medium of the Sanguiferous System. In a Letter to the EDITOR *from* MAXWELL M'DOWELL, *M. D. of Baltimore.*

Baltimore, February 14*th*, 1809.

SIR,

IF, the following singular pathological fact should appear to you worthy a place in your Museum, it is at your service. I am, sir,

Your humble servant,

MAXWELL M'DOWELL.

Dr. J. R. Coxe.

Medical philosophers have, for a long time, suspected the existence of a much nearer *channel* of communication between the alimentary canal and bladder, than the circuitous route of the blood-vessels. Numerous physiological and pathological facts have given rise to this suspicion. Ingenious experimentalists have endeavoured to make suspicion yield its seat, in the human mind, to the clearer evidence of *established* fact. The knife of the anatomist has also been employed in the important investigation; but it has not thrown any other light upon the subject, than what is reflected from a discovery in *comparative* anatomy. The ingenious and ever to be lamented Mr. Hewson discovered in a turtle, " that the lacteals near the root of the mesentery anastomose, so as to form a net-work, from which several large branches go into some considerable lymphatics lying near the spine, and which can be traced almost to

the anus, and particularly to the kidneys." The late Dr. Darwin, supposing that the lacteals in the human system were connected with the urinary branch of lymphatics by anastomosing branches, undertook to account for the communication in question, by his doctrine of the "retrograde action of the urinary branch of the lymphatic system."

The subjoined fact, which took place in my own system, satisfies my mind beyond the *shadow* of a doubt, that the suspected communication *actually* exists.

Early in the autumn of 1798, being in a small village in the state of Kentucky, I felt slightly indisposed, and considered it necessary to take a cathartic. I swallowed *half a drachm* of pulv. rhæi. When I felt the action of the medicine in my bowels, I retired to the forest, for the village did not contain *one* temple dedicated to the goddess Cloacina; and to that circumstance alone I am indebted for the pathological discovery. The first evacuation was *per úrethram*, and the extraordinary colour and consistence of the urine produced a momentary alarm, which yielded to a regret that I was not furnished with a vessel to secure the urine for examination. The urine, in passing over the leaves, left an *earthy* matter attached to them. The next time that the medicine required me to retire, I put a four ounce vial in my pocket. I filled the vial with the first urine that appeared, which retained a slighter shade of the same colour of that which preceded it, and was also less turbid. After letting the vial stand a sufficient time, in a secure place, I poured the liquid off the deposited substance, suffered it to dry, and then ascertained, to my entire satisfaction, that it was a portion of the pulv. rhæi, which I had swallowed a few hours before this turbid urine passed off. It may not be amiss to mention, that, though I had two copious *alvine* eva-

cuations, yet their consistence, together with their manner of expulsion, seemed to show that the rhubarb had no other effect upon the bowels, than *moderately* to increase their natural peristaltic motion.

I have mentioned the foregoing fact to several of my medical brethren. Some of them suggested that the deposited substance might have been a *lateritious* sediment. To such I beg leave to propose the following questions: Would urine, in passing over leaves, leave a *lateritious* sediment behind? Does the *passing* current of urine, containing *lateritious* sediment, appear *turbid?* Is it not remarkable that my urine should not have deposited a *lateritious* sediment at any former period of my life? Is it not equally strange that my urine has never been *turbid* since the autumn of 1798? In fine, is it not unaccountable that the only period of my life, in which my urine deposited a *lateritious* sediment, should be *four* or *five* hours after I had swallowed *half a drachm* of rhubarb? About three years ago I took a *scruple* of rhubarb, in order to see if it would pursue the same course; but it passed through my bowels by purging me briskly. The state of my system, however, when I took the scruple, was very different from what it was in the autumn of 1798. I am entirely at a loss to know how the rhubarb found its way into my bladder. Perhaps the communication between the alimentary canal and bladder, in my system, is a "lusus naturæ." Of one thing I am *certain:* the rhubarb did pass off with my urine, for, before I can disbelieve the fact, I must be taught to discredit the testimony of my senses of SIGHT and TASTE. Surely no man, in the dispassionate exercise of his intellectual powers, could for a moment suppose that the rhubarb entered my left subclavian vein, passed on through the right auricle and ventricle of the heart, performed the round of the pulmonic circulation, entered the

left auricle and ventricle of the heart, and continued its course along the descending aorta, to be conveyed out of the system through the emulgent arteries and urinary organs. Such a supposition would, in my opinion, be an outrage against common sense. Till the knife of some fortunate anatomist dispels the cloud which, at present, envelopes the subject, the only resting place I can find, for my mind, is that which Dr. Darwin has suggested. I must therefore think that a *diseased* action of my lacteals took the rhubarb out of the alimentary canal, and that anastomosing branches conveyed it to the urinary lymphatics, and that it entered my bladder by means of the "retrograde action of the urinary branch of the lymphatic system." Of the truth or error of this hypothesis "*Dies doceat.*"

An Account of a Case of Diseased Uterus and Ovaria. Communicated to the EDITOR, *by* WILLIAM BALDWIN, *M. D. of Wilmington, in the state of Delaware.*

October 28*th*, 1808.

TO endeavour to remove the diseases, and to alleviate the sufferings, of our afflicted fellow-beings, are duties of the physician. To fulfil with effect these duties, we should carefully record all extraordinary cases, by which means every possible advantage may be derived from them, in directing others, where we have failed, to more successful modes of practice, and, where we have succeeded, to follow our example: and thus it is that the *healing art* must continue to be extended and improved. The imperfections of *medical science* remain to be deplored. *The hydra, Disease, in some of its various forms, till baffles all our efforts, and triumphantly bears off its trem-*

bling victims prematurely to the grave! But we ought never to despair. We are equally unacquainted with the extent to which our faculties may be improved in our assiduous investigations, as with the abundant means that yet remain concealed from our view, in the wide field of *medical science*. Let the brilliant successes of a *Darwin*, a *Jenner*, a *Rush*, a *Barton*, and many others, stimulate us to new and more vigorous exertions in the cause of suffering humanity. Tetanus, hydrocephalus internus, and phthisis pulmonalis, have all of late yielded to the power of medicine; and when we reflect that it is not very long since an *intermittent* was considered as an *opprobrium* in medicine, we may very reasonably hope that some or all of those diseases, which still continue to mock our efforts, may, at some future period, yield as readily to the power of medicines yet unknown, as an intermittent now yields to the salutary powers of the officinal cinchona.

Judy Delany, a negro woman, married, and aged about forty years, was born a slave at Newport, in this state, but was set free at the death of her master, some years after. About ten years ago she became pregnant, as she supposed, with her first child; but through the whole course of her supposed pregnancy she continued to menstruate regularly. At the end of nine months she experienced for a few days considerable pain, always accompanied with a *bearing down*, which would recur, as near as she could recollect, every half hour. Her breasts filled with milk, which afterwards gradually disappeared. It was the general opinion, both of physicians and others, at this time, that she was pregnant. Her abdomen continued to be distended, but it remained nearly stationary for several years, and she enjoyed such a share of health as enabled her to labour for subsistence. Five years ago an examination was made by a respectable physician, who could easily introduce his finger into

the *os tincæ*, and substances like *hydatides* presented to the touch. She continued in this situation, her abdomen having gradually increased in size, and she had applied in vain to a number of physicians, till last summer, when she was admitted into the Newcastle county infirmary, near this borough. On being examined by several physicians, the *os tincæ* was discovered to be completely closed, and a hard round substance appearing to the touch like the head of a child was detected in its place. The abdomen was greatly distended, and had of late encreased in size more rapidly. The menses still continued to flow at regular periods, but sparingly; and she informed us that they had never ceased to flow. There was nothing remarkable in the state of her pulse, her appetite was generally pretty good, and her bowels mostly regular.

Mercury was made use of till the mouth was considerably affected, afterwards the external application of tobacco to the abdomen, with a variety of other remedies, but all to no purpose.

The bodily pain which she endured was inconsiderable, but she was under the most distressing apprehensions, from an idea that her womb was filled with living *monsters* of hideous and terrific forms! She was further confirmed in this superstitious opinion from a tremulous spasmodic motion that frequently took place in her abdomen, and which indeed bore some resemblance to the motion of the *fœtus in utero*. She had been under this alarming impression since the termination of the first nine months, at which time she fancied herself so unfortunate as to come under the *dreadful influence of witchcraft!* To shake her faith in this belief, no effort of reason could avail, and she enjoyed a temporary respite from the most

agonizing feelings, under the management of a black man, who, she informed us, had so far stilled the *demons* that haunted her, as that she could sleep much better at night!

The disease continued to progress: her thighs and legs became swelled and *œdematous*, particularly the right; and from the fluctuation that was perceivable, there was now reason to apprehend a collection of water in the abdomen, whatever might be the state of the uterus. But she was still able to walk about; was not emaciated, and her countenance had not that appearance which usually characterizes dropsical patients. Her appetite continued to be pretty good, but she could take but little food at a time, owing to the pain which it produced. A table spoonful of pepper, she informed us, often relieved the pain she felt after eating, and kept the *monsters* down! after which she inclined to be sleepy. The right side of the abdomen was rather the most distended and hard, particularly about the region of the liver, where she complained of some pain.

She continued to walk about till within a few days of her death; and could lie without difficulty in a horizontal position to the last.

On the 20th instant, she was seized with faintiness and vomiting. Her pulse was low and feeble. Some volatile alkali and laudanum were administered, which stopped the vomiting; after which she lay easy and sensible of her situation till midnight, when she quietly expired.

The body was carefully examined on the following morning, in the presence of *Dr. Monro*, and several other respectable physicians.

The extent of the tumour, from the *anterior superior spinous process* of one *os ilium* to the other, over the most prominent part of the abdomen, measured twenty-five inches; and from the *symphysis* of the *os pubis* to the *scrobiculus cordis*, thirteen inches, as she lay extended on her back.

From an idea that the abdomen contained a quantity of water, a puncture was first made in the right side with a scalpel, which penetrated the *uterus*, from whence was discharged the enormous quantity of near six gallons of a fluid resembling pus! It was of a pale dirty colour, slightly tinged with red, and emitted a disagreeable fœtor.

On laying open the abdomen, the *uterus* and its appendages were the first objects that solicited our attention. The *uterus* appeared to have occupied the whole cavity of the abdomen and even a portion of the thorax, as the diaphragm was totally obliterated, and the liver chiefly confined in the last mentioned cavity. It adhered slightly to some of the small intestines, and was in many places *scirrhous*, particularly for several inches round the *os tincæ*, which was as completely closed as though no orifice had ever existed. On cutting through into the cavity of this part, the thickness was found to be two inches. At the place of separation from the *ovaria* we cut through an extent of *scirrhus* not less than three and a half inches in length and two in breadth. We found a number of fleshy substances floating in the cavity of the *uterus*, completely detached, of a soft spongy texture, the largest of which would probably have weighed at least half a pound. The internal surface had a rough jagged appearance; the external was smooth. It weighed, when completely evacuated, and separated from the *ovaria*, six pounds.

The *ovaria* were greatly enlarged. One of them was soft, and upon being laid open was found to contain a white fluid substance resembling curdled milk. It was perfectly free from smell, but imparted a slight sense of pungency to the taste. The other was very hard and white. The *ova*, to the number of fifteen, were more or less enlarged, and there were several others of inferior size. The *fallopian tubes*, and the ligaments were all lost in this deformed mass, which weighed five pounds and a quarter! The spleen, liver, lungs, heart, and indeed the whole of the contents of the abdomen and thorax, had a natural healthy appearance. The stomach was remarkably contracted, but exhibited no mark of disease.

This account shall be closed with a few observations and reflections.

The *uterus* and *ovaria* are liable to be affected with scirrhus, cancer, dropsy, polypi, tubercles, &c. but such a combination of these morbid affections, prevailing to such an extent, and for so long a time, has perhaps seldom been met with. The opinion of pregnancy that was first entertained by the patient and others, was, in all probability, fallacious; and the symptoms that favoured such an opinion were the effect of disease, influenced, perhaps, in some degree, by the state of the patient's mind. Those spongy substances found in the cavity of the *uterus* were probably *polypi* or *tubercles;* and the jagged appearance of the internal surface of that organ, with the state of the fluid therein contained, pointed out the existence of *cancer.* It is a remarkable fact, that the discharge of the *catamenia* continued throughout the whole course of the complaint, notwithstanding the mouth of the *uterus* was so effectually closed. Does not this circumstance prove that the discharge is from the *vagina* instead of the *uterus?* It is perhaps impossible to

account for the destruction of the *diaphragm*, and to satisfy ourselves of the reason why a muscle considered of such importance in respiration could be spared without any inconvenience, as the want of it was not perceived ; no difficulty of breathing having occurred, either in an erect or horizontal position ! Might it not have been absorbed from the pressure of the *uterus?* The motion which our patient complained of, and which might readily be felt by the hand, proceeded, no doubt, from contractions of the distended *uterus*, and might be in some degree influenced from the superstitious idea she entertained of being full of living animals, as she was relieved by remedies which acted through the medium of the mind.

This case affords us a striking example of the extent to which *disorders*, or local affections, may proceed, without inducing *disease*, or general morbid action. A scratch with a bone on the finger, and the puncture of a nail in the foot, have in a few days terminated in death, from general morbid action being induced ; while we find that *cancer* and *dropsy of the uterus*, with *scirrhous ovaria*, may exist for many years, without producing *disease*, or injuring materially any of the animal or vital functions.

Observations on Cold, by Dr. George Williamson. *In a Letter to the* Editor.

Baltimore, December 1*st*, 1808.

Dear Sir,

IN the several last numbers of the Museum, we were favoured with some ingenious speculations and facts on cold. Both of these writers, no doubt, think themselves correct; and I will not adopt the language of the poet, and say, they are "each claiming truth, and truth disclaiming both;" nor yet, that my sentiments are more correct than either of theirs. It may be thought officious in me to address the public on an important subject, which is under discussion by those of superior information. But my wish is not to intermeddle with these; nor do I purpose to enter into an examination of their sentiments, further than as generally received by those who espouse either of their opinions. I am not fond of controversy; too many, who are, have access to irritating and abusive language. To be both disbelieved and abused is more than I am willing to be subjected to: nor am I vain enough to suppose that my information on the subject in question is superior to that of my fellow professors; nor that I shall be able to make use of more powerful arguments than those who have already written on it. But I wish, in as conspicuous a manner as in my power, to present the reader my sentiments; and I intreat him to examine them maturely. If this is done, and done with an unprejudiced mind, he will, if correct, adopt, but, if erroneous, reject them. To me, it would be a source of great pleasure to be irradiated with the superior information of other gentlemen on this or any other subject; and if I possess any information on this subject which my fellow practitioners do not, I wish to impart it to them, because I think it i cur profession

of much importance; and it is very desirable that it should be correctly understood, and not that there should be such a diversity of sentiment. This diversity of sentiment deters many from having access to one of the most valuable remedies we are in the possession of; and should they have access to it, unless they know upon what principle it acts, they may be right, but it is quite as possible for them to be wrong; if they should err, their error is frequently of an alarming if not a fatal nature, as will be proved by and by. Although I am anxious to become completely acquainted with every thing relative to our profession; and not only myself, but that all should who undertake the practice of medicine, and have the lives of their fellow beings entrusted to their care; yet I have so little solicitude for the adoption of my opinion, that, should any person think proper to reply to this essay, I shall be silent, unless I think I can adduce something new; and even should I suppose I can further elucidate the subject, shall not notice an anonymous writer. An anonymous controvertist I consider as an enemy in ambush: if he gains a victory he soon exhibits himself, at least to his friends; but, if unsuccessful, skulks off under the veil of obscurity, and when out of all danger is very apt to join with those who censure his production.

I shall first make a definition which I wish to be understood as my opinion, that I may be comprehended, and that there may be no useless display of words about an undefined proposition.

Every thing that directly reduces the excitement is a sedative.

I shall next lay down two propositions, which I consider incontrovertible, and which are, I believe, generally received.

1st. Caloric, or matter of heat, has always a tendency to support an equilibrium*.

2d. There is in the cellular membrane and muscles an inherent power, which produces contraction when the excitement that expands them is removed.

First. Every thing which directly reduces the excitement is a sedative.

I shall now endeavour to prove that cold directly reduces the excitement. In prosecuting this part of my subject, I shall, in compliance with usage, adopt the term cold in its general acceptation. I am, however, aware, that in speaking of any thing, to say that it has such and such effect is granting it to be an active agent; and that every active agent is a stimulus. It is therefore to be regretted that a term which is so highly improper, as tending to deceive, and distract the judgment, consequently occasion irreparable injuries, should not, at least, be expunged from medical language.

I do not deem it necessary to enter into a detail of facts which are obvious to the most superficial observer, and which are acceded to by those who most strenuously advocate the doctrine of cold being a stimulus: but shall proceed to notice some which, to me, appear more conducive to the support of such an opinion, than any others with which I am acquainted, or that have been advanced by its advocates.

1st. After coming out of the cold bath, there is a glow on the skin, with increased heat, and the pulse is accelerated: but this does not prove cold a stimulus, for cold does not pro-

* Unconfined caloric.

duce this effect. If cold does not produce this effect, the advocates of it being a stimulant will ask, what does? For their satisfaction, I will endeavour to prove it owing to heat. We will suppose the temperature of the atmosphere which surrounds us to be 75° of Farenheit, the bath 60°, and that of our system 96°. When we go into the bath, a diminution of excitement takes place; consequently an accumulation of excitability. While in the bath, if composed, the pulse is less frequent, &c. but on coming out of it into an atmosphere from ten to fifteen degrees warmer than the water we have just left, and that increased temperature operating on an excitability several degrees accumulated, it must necessarily produce an augmented excitement; for heat is a stimulus, and every increased degree of heat increases the excitement, if the attendant circumstances are such as they were previous to this increase. It may now be said, if I do not suppose cold to act directly as a stimulus, that I do indirectly. Objections of this nature would divert me from my proposed plan; therefore, in reply to it, I must first say, that on the present occasion I wish to wave all indirect causes and effects. Second, to say, because heat operates more powerfully on a system whose excitability has been accumulated by cold, that this is a stimulus, would be no more consistent, than to say, ether, spirits, &c. are sedatives, and therefore should be used as such in cases of increased excitement.

2d. Cold is a sudorific; but this does not prove cold a stimulus. When a person labours under a great increase of excitement, if there is an exhalation from the cutaneous capillaries, the evaporation is so rapid as to prevent the aqueous particles from concentrating and remaining long enough on the skin to be discovered. But when a diminution of the excitement takes place, the evaporation is less rapid, and a deposi-

tion on the skin is the consequence. This diminution may be easily accomplished by cold: hence cold is a sedative. In some diseases, it may be said, the skin is preternaturally hot, and there are no obvious symptoms of a diaphoresis, but so soon as one breaks out, the patient is much relieved; and this is accomplished by cold. Such may be the fact, but it does not prove cold a stimulus. If cold is applied externally, there ensues an expenditure of heat, the skin becomes cool, the pulse less frequent, the system is reduced to the sweating point, and it frequently succeeds. When used internally, although there may be no very evident symptoms of increased excitement, except that of the skin, yet it frequently excites perspiration; not from any stimulating property, but perhaps on the principle of sympathy. The most of those preternatural affections of the epidermis are owing to some disease of the primæ viæ. The skin has been compared to an inverted sac, not only extending over the exterior of the system, but also through the interior, and is everywhere furnished with nerves, the vehicles of all impression. We may, no doubt, to these circumstances attribute, in a great measure, that close connection and mutual influence which is well known to exist between the skin and stomach, &c. and it is also, I suspect, owing to this that cold water, when taken internally, so frequently acts as a sudorific. However, if the doctrine of association should be rejected, that of our general principle is sufficient; and the simple fact which takes place in health, on taking a glass of cold water when the system is much heated from exercise or any other means, establishes it beyond a doubt.

3d. Cold sometimes accelerates the pulse; so does bleeding. This is not a stimulus, neither is that. In this respect, I think the use of the lancet and that of cold very analagous. It is in cases of oppressed pulse that the lancet accelerates it. If

cold ever directly has this effect, it is under similar circumstances. They both lessen the excitement, and are both sedatives.

4th. Cold excites absorption. Perhaps it would be more proper to say, cold prevents suppuration. Heat is a stimulus, and increases the vascular action; and an increased action is necessary to suppuration. Any thing that diminishes heat diminishes the action; consequently retards or prevents suppuration. Blood-letting reduces the vascular action; so does cold. That is not a stimulus, neither is this. Cold is also very friendly to transpiration, especially where there is preternatural heat, and has, by this means, a very powerful effect in preventing suppuration. Independantly of its equalizing the temperature of the general system, much of that heat of the phlegmon, &c. is expended by this process. I think it very questionable whether cold promotes absorption further than by preventing suppuration. When in a healthy state, the absorbents are engaged in performing their proper function, which is to absorb fluids from the different parts of the system, some of which are necessary to health, and others offensive; and, were they not removed, would excite morbid action. In cold indolent tumours, where electricity and other stimulating applications are highly useful, does any good accrue from the use of cold? If so, is it not by accumulating the excitability, and enabling subsequent stimulants to act more powerfully? are not the absorbents susceptible of increased action, and may not those fluids which are thus deposited, and which they remove, be capable of exciting this increased action, especially after an accumulation of excitability?

PROPOSITION II.

"Caloric always endeavours to obtain and support an equilibrium; not that it is equally distributed among all bodies, but is dispersed among them according to their degree of mutual affinity." It is supposed by many philosophers that caloric and animal heat are similar. Oxygen supports both, and neither can exist without it*: they are both very diffusive, and always tend to an equilibrium. Doctors Beddoes, Thornton, and others have written ingeniously and lucidly on this subject, and their conclusion is, that "animal heat proceeds from the chemical union of certain parts of our food and oxygen, modified and combined by the proper exercise of the natural animal functions disengaging caloric." That animal heat always tends to an equilibrium may, perhaps, be doubted. 1st, It may be said, that in a state of health, some parts of the system will be cold, while others are warm. This is not owing to any disposition in the animal heat to pervade some parts of the system and not others, but to collateral circumstances. In cold weather, if one part of the system is more exposed than another, the part so exposed parts with some of its heat to the atmosphere. If a part thus exposed perspires more than another, part of its heat is expended by exhalation; and, if any collateral circumstance favours this process, the expenditure is increased.

* Dr. Beddoes, in a letter to Dr. Darwin, in speaking of the effect superoxygenated air had on himself, says, "In no long time I observed in myself a remarkable power of sustaining cold. Except one or two evenings I never experienced the sensation of chilliness, though cold easterly winds prevailed during great part of the time I inspired the superoxygenated air." He adds, "I was not only able to reduce my bed-clothes to a single blanket and coverlid, but slept without inconvenience in a large bed-chamber, looking to the north-east, with the window open all night, and with the door and window of an adjacent sitting-room also open."

If on a cold day we wet our hands, or if in travelling in a cold rain, with gloves on, that receive and retain water, our hands will be much colder than if they were kept dry, or even than if the gloves were taken off. The Arabians, when conducting caravans over the deserts, have the following remarkable method of cooling their wines. They dig a hole, and, having filled it with straw or clay, they place the bottle of wine they mean to cool into the midst of it, having previously surrounded it with wet straw or clay. They then set fire to the straw, and the bottle of wine is brought out (from the evaporation of the wet clay or straw surrounding it) quite cool. Vide Philosophical Transactions, Vol. LXV, p. 252. Other nations cool it by hanging up their bottles in wet cloths to the sun, to expedite the evaporation; the cooling of the wine goes on in proportion to the quickness with which the heat is abstracted by evaporation. It is on this same principle that cold applications are so very salutary in local inflammation, and in some inflammatory diseases of the encephalon.

Our illustrious countryman, Franklin, proved that perspiration always produces a certain degree of coldness. See his letter to Dr. Lind. "We find that almost all fevers end in perspiration; which, besides the advantage of expelling the morbific matter, possesses likewise, that of carrying off the matter of heat, and restoring the body to its common temperature." *Chaptal.*

That almost all fevers end in perspiration is too general an assertion; but that the most of those do, which do not terminate in death, or in which the excitement has not been reduced by some depleting remedy, is an unquestionable truth.

" If heat be applied to water, these two fluids will unite, and the mixture will be dissipated in the atmosphere ; but it would be an abuse of words to call so weak a union by the name of combination ; for as soon as heat becomes in the situation to combine with other bodies, it abandons the water, which returns to a liquid state. This body, during evaporation, continually carries with it a portion of heat ; and hence, perhaps, result the advantages of transpiration, perspiration, &c." *Chaptal.*

According to my position it may be said, that the animal heat must be different in different climates. That the climate has a material effect on the animal system is a truth well known. Dr. Robertson, in his History of America, says : " In every part of the earth where man exists, the power of climate operates with decisive influence upon his condition and character. In those countries which approach near to the extremes of heat and cold, this influence is so conspicuous as to strike every eye. Whether we consider man merely as an animal, or a being endowed with rational powers which fit him for activity and speculation, we shall find that he has uniformly attained the greatest perfection of which his nature is capable, in the temperate regions of the globe. There his constitution is most vigorous, his organs most acute, and his form most beautiful ; there too he possesses a superior extent of capacity, greater fertility of imagination, more enterprizing courage, and a sensibility of heart which gives birth to passions not only ardent but persevering. In this favourite situation he has displayed the utmost efforts of his genius in literature, in policy, in commerce, war, and all the arts which improve and embellish life." These inferences were drawn from actual observation, and are consistent with those of other historical travellers.

That animal heat is influenced by climate can hardly be doubted. Climate has considerable influence on the pulse, and a mutual influence between animal heat and the circulation is acknowledged. Although there are these connections, and although the climate has considerable influence on the pulse, yet it is not in arithmetical progression; nor is there that difference of animal heat in different climates that we might at first suspect, owing, I imagine, more to artificial than any natural cause.

This opinion is rendered at least plausible, from abundant experience, not only in man, but also in inferior animals. In climates colder than the temperature of the system, men have access to clothing, &c. to prevent such an escape of heat from the body as would be unpleasant or injurious; and He who regards the least of his work, has furnished inferior animals with a warmer covering in those situations than in warmer climates. Their covering is not only warmer, but is also a bad conductor of heat. Hunter endeavoured to freeze a dormouse; but at first found he could only freeze its feet. Supposing the hair to be a bad conductor of heat, he, in his second trial, took care to wet it all over: after this he succeeded.—*Hunter, on the Animal Economy.*

Feathers, when dry, are also bad conductors of heat. Birds will fly through very cold regions, when dry, more so, perhaps, than when wet. And yet Providence has not been unmindful of aquatic fowls. The effect cold rain has on the common dung-hill fowl is strikingly obvious.

There is also a peculiar economy observed by man and inferior animals to prevent, in cold weather, too great an expenditure of heat: observe, for instance, the dog. If the weather

is very hot, he extends himself in the shade, his mouth wide open, and respiration hurried. If the weather is very cold, he endeavours to get as near as possible to a fire. If this cannot be accomplished, he gets into the sun, or the warmest place he can, and contracts himself into the smallest possible dimension, knowing, from painful experience, that the more he is extended in cold weather, the colder he will be. These observations are also applicable to the cat, &c. The hog, in hot weather, hunts for a mud-hole or water. When they are wet, the expenditure of heat is much increased; in cold weather they huddle together in heaps, having experienced that there is more heat in several than in one. In fact, there is no part nor inhabitant of the terraqueous world, which comes under the operation of severe cold, that does not show, conspicuously, the print of her congealing hand.

Some substances conduct heat less rapidly than others; so do some animals: those that do not perspire impart with it less than those that do; and those that do not impart much heat this way, impart more by respiration than those that do; and "we know that the heat contained in one breath of air, will, if properly managed, raise Farenheit's thermometer ten degrees*."

Animals retain the heat while in a contracted state, and expend it while extended with the mouth open. Horses, dogs, &c. in cold weather, have access to muscular motion: such as running, leaping, playing, &c. which increases respiration and heat. Muscular motion has the same effect with man.

* Vide Critical Review, for January, 1782, page 6.

The weather has also great influence on the retention and expenditure of animal heat. If the weather is dry and calm, the expenditure is much less than in windy wet weather, even if the temperature is precisely the same. Moist air is a better conductor of heat than dry, because water, though of the same temperature of air, will carry it off more rapidly. The effect produced by fanning will serve to elucidate that occasioned by the wind; the latter is more active and more extensive in its nature, it carries the warm air to, and brings the cold from, a greater distance.

When these two causes unite, we may easily comprehend why the heat from animals should be carried off more quickly, and they experience a greater sense of cold, than in still dry weather. This also enables us to account for the peculiar disagreeable effect moist windy weather has on our system in the colder seasons of the year. The constant production and succession of heat in the animal, and its tendency to pass off by the surface, and this being influenced by collateral circumstances, prevents the animal heat from acquiring the temperature of the air, and from becoming like that, stationary, varying only with the real changes that take place in the atmosphere. The sense of cold felt by us must be owing to the constant escape of heat, and the degree of cold to this escape, and to the celerity with which the air is enabled to carry off the warm atmosphere surrounding us.

That the temperature of the system can be augmented and supported by artificial means, is an unquestionable truth; and that an expenditure of it may also be increased is no less true. Thus, "when heat is accumulated in the system, either by fever, by strong exercise, or by the scorching heat of the sun, nature constantly cries aloud for acids and a cooling diet.

Acids, when taken into the stomach, always check and restrain the generation of heat, or, in other words, when the system is saturated with oxygen only, less oxygen air (oxygen and caloric) is imbibed by the blood in the lungs, and consequently less heat will be evolved in the body. It is upon these principles," says the Rev. Mr. Townsend, " that the reapers in the south of Spain covet their guzpacho, composed of bread, oil, and vinegar: the two first articles for nutriment, and the latter to moderate their vital heat. On the same principle, obedient to the voice of nature, during the sultry heats of summer, we equally desire our lettuce, oil, and vinegar."—*Medical Extracts*.

Oil not only serves for nourishment, but has also a tendency to relax the solids, as is experienced by those who make much use of it: this is very obvious in parturition. On this principle it may promote perspiration, and hence reduce the temperature of the system*. If my position, " that heat always tends to support an equilibrium," should be objected to, because we can live in a temperature warmer than that of the system, as is proved by those employed in glass-houses, founderies, &c. I would reply, although this is a fact, yet I am not certain that any person, in such a situation, will continue to enjoy good health; and there is no doubt but those thus situated are shorter lived than those in more healthy situations; and, while in this increased temperature, there is a continual increase and diminution of excitement, and the temperature of the body is equalized and moderated by perspiration. Some substances have a greater capacity to receive and retain

* On conversing with an intelligent gentleman on this occasion, he told me he believed I was correct, although the idea was new to him. He observed, that last summer he had eaten much oil, but never before, and that he experienced less inconvenience from heat than formerly.

heat than others; some to receive it, others to retain it, and others to impart it. Spirits boil at 176° of Farenheit; water at 212°; at 75° the animal system is pleasant, and there is emitted an aëriform exhalation; and at a higher degree an aqueous one. Although there is a mutual affinity between animal heat and the temperature of the atmosphere; yet I do not think it necessary to health, that that difference which generally exists, should always be precisely the same; or that either an increased or diminished atmosphere of ten degrees should always produce a similar effect in the animal heat. To say that heat always tends to an equilibrium, is not saying that it always supports one; nor is asserting that there is a tendency to support one, as established by the God of nature, saying that this is always done: my position is, that, when a departure from this general rule takes place, there is a tendency to re-establish it. We know that collateral circumstances have great influence on animal heat; also, that there is in different bodies or substances different capacity for receiving, retaining, or imparting it; and further, that, when the animal system becomes oppressively hot, that it is relieved by perspiration, &c. May not this also prevent an accumulation of heat beyond a certain degree, or, if it is accumulated beyond that degree, will not immediate death be the consequence? When a violent stimulus produces great increased excitement, unless this can soon be reduced, an apoplexy, &c. is the consequence. Heat is an active stimulus, and produces increased excitement; and, when prevented from escaping by its natural outlets, no doubt occasions apoplexy, &c. I cannot believe, because a person who is in a temperature of 120°, and his own is not that, that heat is not a stimulus; nor can I believe, because a person is in one of but 12°, and he has a temperature of 90° or more, that cold is not a sedative. Where there is an increased temperature of the atmosphere, there is

an increased animal heat; if it is so great as to be disagreeable, we assist nature in endeavouring to reduce it. When the temperature is considerably diminished, we have access to artificial means to support it; if these fail, and it continues to diminish, a torpor, sleepiness, and death is the consequence.

The fate of sir Joseph Banks, Dr. Solander, and others, in their botanical excursion on the heights at *Terra del Fuego*, proves the effect of cold on the system. Dr. Solander had more than once crossed the mountains which divide Sweden from Norway, and knew well the effect of its extreme cold. He therefore conjured the company to always keep in motion: "whoever sits down," says he, "will sleep, and whoever sleeps will wake no more." The doctor was, however, overcome with cold, and wanted to lie down; sir Joseph remonstrated, but down he lay. His friend, with great difficulty, kept him from sleep, and, partly by persuasion, and partly by force, he was induced to go forward. Soon, however, he declared he would go no further; sir Joseph again expostulated; the doctor said he would go on, but must first take some sleep, although he had told them "to sleep was to perish." In a few minutes he fell into a profound sleep, and, after five minutes, sir Joseph happily succeeding in waking him, he had almost lost the use of his limbs, and the muscles were so shrunk, that his shoes fell from off his feet. Every attempt to relieve an unfortunate black man, who was similarly affected, proved unsuccessful.

"The ten thousand Greeks, in their memorable retreat in passing through Armenia, were exposed," says Xenophon, "to a contest still more dangerous than the enemy, in which neither skill nor valour could avail. The snow fell in such quantities through the night, as to completely cover the men

with their arms. Their bodies, when freed from the snow, were benumbed and parched with the piercing coldness of the north wind. Many slaves and sumpter horses perished, with about thirty soldiers. It was observed that those died who did not use sufficient exercise." But why go so far from home for facts to prove this melancholy influence of cold, when every winter furnishes us with so many cases in our own country? Can these effects be attributed to any stimulating power which cold possesses?

Cold, under some circumstances, exhilarates; but how? not by any stimulating property it possesses, but by arresting too great an expenditure of excitement. It may be asked, can cold both cause and arrest an expenditure of excitement? Under the last head, I observed a wish to wave every consideration of indirect causes and effects; however, as I have been inadvertently drawn into this remark, I deem it expedient to take some further notice of it.

We know that some medicines have both a direct and an indirect effect. Opium is directly a stimulus, and indirectly a sedative; so is ether, spirits, &c. and so is heat. After being in a temperature of 25°, if we go into one of 50°, we shall be exhilarated, and experience those pleasant sensations occasioned by an agreeable stimulus operating on an increased excitability. But let this heat be augmented so as to produce an excessive perspiration, and let this be supported for some hours, and we shall be much exhausted; but, on going into a cooler temperature, will be refreshed; the stimulus will be less, and there will not be so great an expenditure of excitement; there will also be an accumulation of excitability, and the subsequent necessary stimulants will have a more healthy operation on the system: such is the effect as regards summer.

After several months hot weather, we are pleased to feel a cool evening: it arrests excessive perspiration, and enables the relaxed muscles to regain their healthy tone; not by stimulating, but by preventing too great an expenditure of excitement, &c.

Independently of the effects experienced on the body through the influence of animal heat, there are also other causes which materially affect the system, even when in health. When the temperature of the atmosphere, &c. are precisely the same, both our mental and corporeal system are materially affected. There is a very close connection between the two; and when one is in a morbid state, the other frequently participates. There has been supposed a very great analogy between the nervous and electric fluid; the latter is greatly under the influence of the weather, and we cannot doubt but the former is as much so. The nerves are the seat of all sensation, and all impressions are made on and conveyed through and by them. If the nerves are the vehicles of all sensation, and they, like electricity, influenced by the weather, ought we not to attribute in some degree our peculiar feelings to their direct influence, and to the indirect influence of the weather, &c. through them?

I have dwelt longer on this part of my subject than was intended; but its importance in a medical view, and a wish to elucidate so ambiguous a part of our science, will, I hope, guard me from censure. I shall now proceed to explain some of the phenomena peculiar to the practice of medicine, on the principles which are here held forth.

1st. In syncope, cold is supposed very beneficial.

What is the cause of syncope? Is it ever owing to cold, or are the effects of excessive cold and syncope similar? Is not this disease more frequently occasioned by heat than any other cause? When heat is the cause of it, our theory is immediately comprehended. But it is also produced by severe injuries, loss of blood, impure air, &c. What is the condition of the general system immediately preceding it from these causes? Let syncope proceed from what cause soever it may, is there not, directly preceding it, a great exertion in the general system? does not the universal and copious sweat pronounce an increased heat? It is immaterial what this heat is owing to, whether re-action or any other cause: when cold is applied, it immediately tends to equalize the heat, to accumulate the excitability, and prepare the system for other remedies. It also, by causing an expenditure of the superabundant heat which relaxes the cutaneous vessels, enables them to contract and perform their ordinary function. A similar effect is produced in the lungs; but are we to depend solely on cold in syncope? If the cause is not removed, will the effect cease? When it is in consequence of heat, cold removes the cause. If it is from injuries, loss of blood, contaminated air, &c. will it cease while the cause continues? and when the cause is removed, do we not often find it necessary to have access to other remedies than cold? such as ether, volatile salts, &c. These do not operate as cold; it, diminishes the excitement, and causes an accumulation of excitability, and they, are stimulants; and stimulants act with increased energy on an accumulated excitability. When the patient begins to revive he calls for volatiles, &c. to stimulate the nervous system, and their effect is conveyed through this channel to the general system. He also begs the pure air to be admitted, and to be washed with cold water. Why? because they cool and make him feel pleasant. Do stimulants directly produce this effect?

2d. Cold is salutary in asphyxia.

The observations under the last head, respecting the operation of cold, are equally applicable here. When asphyxia is occasioned by *carbonic acid gas*, and cold water is applied, has it not a tendency to correct the atmosphere by absorbing some of the noxious gas*?

The explanations given under this proposition enable us more satisfactorily to account for some of the phenomena, partially explained under the first, relative to the use of cold in fevers, &c. and, were it not a work of supererogation, we might now proceed to notice its effects in typhus fever with preternatural heat of the skin; also in the typhoid state of fever, cholera infantum, &c. Its effect in the synocha and synochus states of fever is generally comprehended.

PROPOSITION III.

There is inherent in the muscles and cellular membrane, a power which produces contraction, when the excitement that expands them is removed. Haller says, " the muscles possess a contractile power, by which they both resist extention, and, when the extending power is taken away, acquire their former shortness; nor does this power ever cease endeavouring to

* Water receives, when agitated, &c. its own volume of carbonic acid gas, and it is very abundantly absorbed by a solution of pure potash. These facts dictate some important truths, advisable to be attended to, both in prescribing for persons affected with this gas, and also in correcting it. Lime has an extremely strong affinity for carbonic acid, which enables it to take this acid from other substances. Lime, when exposed to the atmosphere, first acquires moisture, and then carbonic acid. Hence, in part, the discolouration of white-washed walls, &c.

bring the elementary particles into the closest contact the mechanism of the part can admit. After death, even for many days, it does the same; so that fibres of a divided muscle contract towards each extremity, and leave a wide gap in the middle; also arteries, when divided, contract themselves in length." This force he calls dead, because it continues after death, and is so far different from those powers of life. In the living animal they are more lively than in the dead one. Again, the cellular fibres are perpetually endeavouring to shorten themselves, and always tend to their own contraction. Hence, when the skin or any other membrane is extended, as soon as the cause of extention is taken off, it returns, by a gentle effort, to its former shortness. It is the nature of this power to act continually by a gentle but uninterrupted effort." *Haller's Physiology.*

Blumenbach, in speaking of vital energies, says, "the first and most universal species of those vital energies, and which may, indeed, be regarded as an inferior degree of the others, or rather as the threshold leading to them, is simple contractility; i. e. a propensity in a part to contract itself." Contractility he supposes peculiar to the cellular membrane, and that it pervades almost the whole body. That this membrane possesses contractility, he thinks obvious, from the construction of the *dartos tunic*, the spasm of the skin, or from the peritoneum, which alone appears sometimes to incarcerate and strangle the intestines in cases of hernia. He supposes this energy so extensive as to even include the bones. See his *Physiology.*

Although I have great veneration for the charcter of Blumenbach, and put much confidence in his judgment, yet I believe the muscles as well as cellular membrane possess this

energy; however, either of the doctrines are sufficient to explain my principle.

That there is inherent in the system such a power, has been proved by many experiments, and is daily proved by incontestible facts.

Dissect a muscle from the body, and stretch it by manual force or any other means, and so soon as the cause of extension is removed, the muscle will contract. In antagonist muscles, if one of them becomes injured, so as to lose all power of contraction, the other contracts the limb. After death, the muscles or cellular membrane continue to contract until the body, which was quite relaxed, becomes very rigid and smaller. Were it requisite, instances of this kind could be adduced *ad infinitum*.

There is inherent in the cellular membrane and muscles a power which produces contraction when the excitement that expands them is removed. Here orignate two questions of some magnitude.

1st. What is it that expands the muscles?

2d. How is this expanding power removed?

First. If the muscles are not directly and entirely expanded by heat, it is a very powerful agent in this process; nor can the expansion be continued without it. How does heat produce this expansion?

It is one of the established laws of physics, that heat expands every body or substance into which it is admitted; whether aëriform, fluid, or solid. The blood is considerably rarefied by heat, and is consequently the easier propelled into and through the minute vessels. That the blood-vessels are expanded by heat may be easily proved. Deprive any muscle in the body of heat, and the vessels which are distributed through it will soon become impervious; and, if the blood does not continue to circulate through any muscle, in an ordinary manner, it will soon be deprived of part of its heat. In old age, many of the cutaneous vessels, if not entirely impervious, are so much so, as to perform much less completely their function, than at an earlier period in life; the general circulation is also slower. Hence, old persons possess less animal heat than the middle aged, and these than the younger. "Now king David was old and stricken in years; and they covered him with clothes, but he gat no heat." The next remedy prescribed was a young virgin, who was to stand before the king and cheer him, and to lie in his bosom that he might get heat.—I Kings, Chap. i. That persons who have much animal heat impart it to those who have less, is a fact generally known; and that young persons possess it in a greater degree than old, is not less so. The modern theory of animal heat is so well known, not only as respects its generation, but also its evolution through the whole system, that to dilate on this part of our subject would be trespassing on the reader.

If heat has this effect in the expansion of the muscles, the answer to the second query, How is the cause of that expansion removed? will be, by removing or expending the heat. This explains more completely our doctrine; and that this is the fact I have not a doubt. Heat has always a tendency to support an equilibrium; hence it is very obvious, that, when

cold is applied to, or comes in contact with the animal system, it must occasion an expenditure of the former. The muscles and cellular membrane always contract when the excitement that expands them is removed. Heat expands every thing into which it is admitted, consequently expands the muscles, &c. hence, when the heat which expands them is expended, a contraction must ensue.

In the winter, when the heat of the system is expended by cold, there is a paucity of blood in the cutaneous vessels; hence the cause of cutis anserina. The shivering which takes place in cold weather is not owing to any stimulating property of cold, but from a diminution of excitement, and from the contraction of the skin, &c. in consequence of this diminution. If this "muscular action," as it has lately been called, were owing to the application of a stimulus, and this stimulus were cold, the action would be increased in proportion to the increase of cold, which is not the case; but if, according to our position, there is in the cellular membrane and muscles an energy which produces contraction when the expanding power is removed, and this expanding power is heat, then this sensation can be satisfactorily accounted for. It has already been proved, that the animal heat is expended much more rapidly when the atmosphere is damp and windy, than in one which is dry and cold, even if in the former the weather is much the warmest. Hence I conceive it obvious, that this action is not in consequence of any stimulus, but owing to an expenditure or diminution of excitement, and a contraction of the skin, &c. in consequence of this diminution; and that this may be occasioned by cold, but more rapidly by a cold wet air than a dry one, even should the former be several degrees the warmest. This effect is not peculiar to the contraction of those parts which come in contact with cold; for frequently

when the bladder has been much distended, and after its contents have been evacuated, as the sphincter vesicæ contracts, there is felt a general tremor, similar to that experienced by cold. But it is not owing to cold; if it were, it would not be confined to the moment of this muscle's contraction. As the cellular membrane as well as the muscles possess contractility, the constriction of the dartos, &c. can be satisfactorily accounted for, without having access to the aid of the supposed stimulating property of cold.

I have not deemed it necessary to institute any experiments on the present occasion. As to matter of fact, there is no subject with which we are better acquainted than that of cold. But we have been at a loss how to account for its operation on philosophical principles. The many experiments that have been made by various persons do, I conceive, prove the correctness of my opinion. Those lately instituted by Woodhouse, Klapp, and others, are very forcible; and they who have considered cold a stimulus, have fallen into some egregious errors.

Currie's Treatise on Cold Affusion, &c. is certainly an invaluable work, but his principles are erroneous, and, in some cases where he was influenced by them, he committed errors of an alarming nature.

In 1792, he, in company with Mr. Hoffman, an ingenious Prussian surgeon, visited a patient on whom they were going to use the cold affusion. When they went to see him he was in the cold stage of a fever. "He was taken out of his bed shivering, his pulse small and frequent, his extremities shrunk and cold. In this state cold brine was dashed over him as usual, but not with the usual happy effects; his breathing was

for some minutes almost suspended; his pulse, at the wrists, was not to be felt; the pulsations of the heart were feeble and fluttering; a deadly coldness spread over the surface; and when respiration returned, it was short, irregular, and laborious. After the use of frictions on the surface, and particularly on the extremities, of a steady warmth, applied some time to the scrobiculus cordis, and of cordials cautiously administered in small quantities, the pulse of the wrist returned; but for some time it was excessively quick and feeble." Currie, from facts of this alarming nature, learned, "that the cold affusion could not be used with safety during the cold stage of the febrile paroxysm; also, that great precaution is necessary in the use of this remedy, and that it ought never to be forgotten, that an application of cold, which is safe in the violence of a fever, is not safe when the fever is removed; and that injuries have sometimes occurred from continuing the cold affusion in the period of convalescence; and further, that it is not safe after the sweating stage of fever has continued some time, and the body is passing through that cooling process." See his treatise.

After this view of the subject, I conceive I may, with propriety, draw the following inferences:

1st. Every thing which directly reduces the excitement is a sedative:

Cold directly reduces the excitement;

Therefore cold must be a sedative.

2d. Caloric or matter of heat has always a tendency to support an equilibrium:

Caloric and animal heat are the same;

Therefore must operate on, and be influenced by the same principles.

3d. There is inherent in the cellular membrane and muscles an energy which produces contraction, when the excitement that expands them is removed :

Heat is materially interested in expanding them, and it is not accomplished without ;

Therefore they must contract when the animal heat is expended, or when there is a diminished excitement.

Two Anomalous Cases of the Venereal Disease. Communicated to the EDITOR.

Monmouth County, New Jersey, November 10, 1808.

SIR,

THE following two cases may, perhaps, not be entirely uninteresting to some medical gentlemen. If you deem them worthy of a place in your valuable Museum, I shall feel gratified at their obtaining publicity through so respectable a channel.

Yours, with respect, &c.

W. G. R.

Dr. John R. Coxe.

In 1801, I was attached to a United States' ship, of the squadron destined for the Mediterranean station. In our passage across the Atlantic, and upwards of five weeks after our men had ceased to communicate with the American sea-ports, William Cogan, seaman, complained to me of a swelling in his left groin. As the venereal had, for some weeks previous, been a very common disease among a number of the ship's crew, I suspected his complaint to be of similar origin; but, on examining, I could discover no topical affection, nor any other symptom of disease than an induration in one of the glands, without discolouration. He said he had not observed any sores on, nor discharge from the penis, at any recent period; but that, about a week before leaving Philadelphia, he had indulged in sexual intercourse with some of the *girls of the town.*

As no visible mode of his receiving the disease could be discovered, I rather viewed his complaint as the offspring of cold, or of some other cause of glandular obstruction, less serious in its effects than the venereal disease. I dismissed him for the time with some slight application, and a direction to return if the complaint became more troublesome.

About twelve days after, he returned. The tumor had then acquired the size of a hen's egg; was hard, of a lively red colour, and painful to the touch, with sharp pains darting through it. I directed an emollient poultice to the parts, to be renewed two or three times each day; enrolled him on the sick list, and returned him off duty. Six days after, the tumor burst, and discharged a matter resembling, in colour and consistence, the matter discharged from an ordinary bubo. Soon after the tumor burst, the tumefaction and quantity of

the discharge abated to a considerable degree, and in three or four days the matter had degenerated into a thin sanies, which wept from the sore, and seemed to prevent the healing or closing up of the same; at the same time the soreness and rigidity of the parts had so far abated, as to enable him to do light duty on board the ship.

For ten days after his return to duty, the ulcer continued to retain much of the last named appearance, and I remained undecided in respect to the most proper method of its treatment. I was aware of the various anomaly in the symptoms of complaints *purely venereal;* and that a primary affection was communicable by absorption alone, without any external ulceration, was an opinion I knew to be admitted by very respectable medical authority, but an opinion I supposed to owe more of its existence to anatomical or physiological inference, than to any well-grounded experience on the subject. I had never met with any case, or well authenticated history of one, that placed the matter beyond doubt.

About the expiration of the last named period, he informed me that he felt a hard lump, with pain, in the right groin. I examined and found two indurations seated in the lymphatic glands of the penis. I marked their daily progress for about eight days, at the end of which time they began to be considerably enlarged, inflamed, painful to the touch, or, on moving the limb, accompanied with a slight acceleration of pulse.

The true character of the disease was now too conspicuous to be mistaken. I directed mercury (principally by inunction) in such quantity, that on the sixth day of its exhibition a ptyalism was perceptible. The tumors remained stationary for several days, then began to diminish in tension and magni-

tude, to be less painful, and finally to disappear by the twentieth day from the first exhibition of the mercury. On the twenty-fourth day the ulcer was entirely healed, and on the twenty-sixth the medicine was discontinued, at which time every symptom of the disease had disappeared. A slight soreness of the mouth (which had been constantly kept up) soon subsided, and the patient was enabled to return to active duty with his accustomed health and spirits.

Four months after his recovery he contracted the venereal disease in one of the Italian sea-ports. An extremely obstinate chancre was the first symptom, which was succeeded by a bubo equally obstinate: the latter was strikingly similar, in every stage of its progress to maturation, to the buboes above mentioned.

About the 20th of May, 1808, P. F. with visible trepidation of mind, complained to me of what he feared to be a venereal infection, contracted about four weeks before in New York. He said he had for several days past felt pain in the urethra, sometimes darting along the perinæum, and in his hips, running thence to the calves of his legs, with obtuse pain in the shoulders, communicating to the arms. No symptom of disease was apparent on or about the genitals, nor had he discerned any from the time he supposed the complaint to have been contracted. Observing the state of his mind, and no other evidence of disease discoverable, I, in my mind, referred his complaint to that variety of the disease which our worthy professor Dr. Rush has sometimes, in his lectures, denominated *noddle pox*, dismissed him with a placebo, and assurances that he would be relieved in a week. At the expiration

of that time he returned, said all his pains were more severe, with occasional titillation at the extremity of the urethra. I now thought that an incipient calculus might possibly occasion the titillation in the urethra, and an alarmed imagination create every other symptom. I gave some gentle laxatives, directed demulcent drinks, and dismissed him for another week. Before this week expired he returned, said he had repeatedly discharged blood from the urethra since he last saw me; and that, in addition to the above symptoms, he had occasionally giddiness in his head, pains shooting across his breast, hurried respiration on moderate exercise, broken slumbers, and, in short, a feverish restlessness throughout the whole frame, without being able to point out any particular seat of disease. I caused him to void urine in a vessel containing water, when two or three clots of blood, the size of a pea each, were discharged with the urine; I searched with an elastic catheter, but could find no part of the urethra preternaturally tender. There was sometimes a slight obstruction to the urine on his first attempting to void it; but the inclination to void it was not more frequent than is common in a healthy subject. There was no tenesmus, priapism, nor enlargement of the prostate gland; his pulse was small, with a little tension in it.

During a long and extensive acquaintance with the venereal disease, practitioners must occasionally meet it under various disguised forms; but it is rarely, I believe, that the most insidious cases may not be detected either by their previous history, a topical affection, some antecedent similar case, or the presence of desultory symptoms, among which experience can discover the venereal as an ingredient. As to the case in question, I now strongly suspected its venereal origin; but, to adapt a successful mode of treatment thereto, was a more embarrassing task: it seemed like attacking an enemy in am-

bush, who assailed you from all quarters, without discovering in what direction his principal strength lay. Suffice it to say, that the symptoms abated much within three weeks, under the copious use of decoct. sarsaparillæ, emulsions of pearl barley, a light soup diet, aperients, a daily moderate use of tinct. g. guaiac. and warm injections into the urethra, composed of sweet milk, and milk and almond oil, blended with a few drops of tinct. opii added to each injection. From the end of the third week, the sarsaparilla and barley emulsions only were used, with a few of Dover's powders daily. At the end of the fifth week, every vestige of disease had vanished, and invigorating diet was recommended, which soon recruited the patient.

To the young and less experienced physician, the former of the above cases presents two considerations of much practical utility. The first is, that whereas the venereal poison may be communicated without any visible marks of inoculation, the absence of the signs by which we generally judge of its presence ought in no wise to prevent the use of proper remedies, when the affection of the system sufficiently points out the nature of the disease. Secondly, as the seeds of the disease may lurk for a length of time in the system, without affording any evidence of their existence there, this fact ought to influence the practice in the treatment of recent local affections: during the cure of these, to preclude every possibility of the lodgement of the virus in the system, by a well-directed course of such medicines as experience declares to be the most proper for that purpose, must be acknowledged a prudent and judicious practice.

The second case offers a question to the etiologist, viz. What was the particular modus operandi of the virus in the

system? Did a lodgement of the poison in any part of the urethra affect the more distant parts of the system by sympathy, or did it enter the circulation through the absorbents of the urethra or glans penis, thence affecting the solids in a manner peculiar to itself, analogous to mercury in a salivation, or is there a still more rational mode of explaining its operation? Without attempting to decide this point, I will remark, that if the idea of absorption is admitted, it will furnish an argument in favour of the distinctive natures of gonorrhœa and lues venerea, as no symptom of lues appeared during the whole complaint.

Case of Monstrous Birth. In a Letter to the EDITOR, *from Dr.* STRYKER.

Somerset, New Jersey, February 28th, 1809.

SIR,

IF you conceive the following case merits publication, I shall be gratified to find it obtain a place in your Medical Museum.

I am, sir, with esteem,

Your most obedient servant,

P. J. STRYKER.

Dr. J. R. Coxe.

—

On the —— day of ———, I was requested to visit Mrs. F. in labour. Her pains regular and of sufficient force, the os tincæ handsomely dilated, and much water protruding the

membranes; in the intervals between the pains endeavoured to ascertain what part of the child presented; but could only satisfy myself that the presentation was not natural. I determined to introduce my hand, break the membranes, take advantage of the abundance of water to examine more accurately the part presenting, and then act agreeably to the exigencies of the case. The presenting part was, in fact, what must be termed the head, and the child was delivered with much facility: it proved an uncommonly large female, and, at the instant after delivery, lively and strong; insomuch that, before I had time to reflect, feared it would survive. The whole of that part of the cranium or brain case, with its usual contents, which is naturally covered with hairy scalp, was absolutely wanting, and the foramen magnum occipitis covered with a bloody excrescence; its face and every other part perfectly formed. It lived, perhaps, three or four minutes, rolling its large full eyes about most part of the time. This was Mrs. F.'s fourth child; all daughters.

Mr. F. was a gentleman of uncommon vivacity, and frequently, during Mrs. F.'s pregnancy, when he came in her room and found no third person present, would walk up to her, seize her cranium with his fingers extended, and observe humorously, "that if she presented him with another daughter instead of a son, thus he would pinch it in the head."

Mrs. F. was a nervous lady, and this farce, frequently repeated, at length gave her so much pain or (rather) horror, that she informed Mr. F. of her sensations, and intreated him to desist.

This account I received from both Mr. and Mrs. F. the latter of whom does not even at this day know that there was any deficiency in the formation of the child.

Query. Could the conduct of Mr. F. (as above related) have had any share in producing the imperfection of this child?

A Case of Sudden Death, with a Singular Appearance of the Lungs on Dissection. By W. G. R.

Monmouth County (N. J.), April 5th, 1809.

ON the second ult. I was requested to visit David Crawford in much haste. Unavoidable circumstances protracted my arrival at the place for nearly three hours after his attack. I found him dead, and the following was the account given me of his case. He had sat down to supper in a very cheerful mood, and, on taking a mouthful of food, began to laugh heartily, though not immoderately, when all of a sudden his voice stopped. A gentleman seated at table with him, observing him make several efforts to breathe, asked him if he was *choaked;* but receiving no answer, rose to assist him. By the help of a servant the gentleman raised him from the chair, when he sunk almost lifeless from their hands, and was extended on the carpet: here some half-fetched respirations, frothing of the mouth, and convulsive heavings of the chest, marked his exit in little more than ten minutes after his first attack.

Mr. Crawford had reached the sixty-second year of life, and enjoyed a greater share of robust health than is common at that period; his conformation of body was apparently

faultless, and his appearance, when I saw him, indicated sleep rather than death.

The circumstances attending this disastrous occurrence, inclined me to consider it a case of strangulation by the inhalement of some particles of the food into the trachea, or bronchial ramifications; and here I would ask, does the faculty of medicine possess any generally successful mode of treating accidents of this kind? I must acknowledge myself unacquainted with any that are either practised or recommended; and I have known a physician to remain an impotent and almost idle spectator, in more than one or two distressful cases.

I am of opinion that the danger or risk of life to the patient, is in proportion to the stage of inspiration at which the offending material is taken into the lungs. If at or near the end of an inspiration, any extraneous body, as a crumb of bread or drop of water, finds its way through the glottis, the convulsive efforts in the respiratory muscles that immediately follow, expel the air with vehemence, and the body is extruded by the current thereof. If, on the contrary, the lungs are nearly empty at the time of receiving the accident, the efforts in the muscles are equally powerful; but, as there is no expelling power within, the cause necessarily remains; and on every endeavour to inspire air (which is apt to be attempted with too much hurry and force) these convulsions are accelerated or renewed, until at length the system sinks for the want of those principles* of atmospheric air so essential to its

* I say principles, because I have for some years past indulged an opinion, that something more than merely supplying the system with oxygen, or freeing it of carbon, is the object of respiration. Some experiments relative to this subject are going forward, which, when sufficiently matured to meet the public eye, I will offer to the Museum.

continuance in a state of life and motion. Several plausible considerations incline me to believe, that not only in occurences of the above kind, but in convulsive laughter, in drowning, and respiring some of the gases, the system incurs more danger by the sudden and total privation of atmospheric air, than in any paralysis consequent to convulsions or other violence done to the system by the vehement action of the heart and arteries.

The first and perhaps only indication on the introduction of a foreign body into the larynx, is to remove the offending cause. How is this to be effected? Tracheotomy, unless indicated by the size, position, or qualities of the body, must be deemed a very unwarrantable mode; and the practice of giving emetics, which I have heard some physicians suggest, is too absurd to require refutation. Inflating the lungs by means of a pipe introduced into the glottis, or even by blowing through the patient's nostrils, might afford some advantage. I know objections may be offered to this practice, but, in my opinion, they are too trifling to forbid its application when the life of an individual is depending, and every other prospect of succour is cut off; it may be employed with equal propriety and more ease, after the system has ceased to re-act. Indeed, I think that attempts at resuscitation, in cases of long suspended animation from this cause, offer as fair a prospect of success, as in like cases of the same standing from any other cause. Should attempts to resuscitate be objected to, under the supposition that they would only subject the patient to the misery of dying twice, by reason of the offending cause, when it remains, reiterating its effects upon the respiratory muscles, as soon as the principles of life return: to these objections I would reply, that, as far as my experience goes, the human

body, on recovering the spirit of animation after a long suspension thereof, does not immediately require its wonted aptitude to motion, or susceptibility to impressions on any of its parts, but is less excitable for an uncertain length of time; in some instances for many days. Under these circumstances, any extraneous matter lodged in the larynx, might be thrown out by moderate coughing or other natural efforts of the organs subservient to respiration; and should such matter be impacted into any of the terminal ramifications of the bronchia (which might easily happen during the resuscitative process), it would either be dissolved by the natural moisture of the parts, or excite suppuration; and, in either case, would be thrown off by expectoration.

These ideas accord with experience, and, I think, with the known laws of the animal economy. I can adduce a living instance (and there are many more no doubt) in which the fragments of a cartridge paper, lodged in the lungs by a gun-shot wound, was discharged many months after the injury had been received, by an expectoration of purulent matter; and another, in which cheese-like concretions were expelled from the lungs after the patient had laboured under a diseased and very weak state of those organs for a period of two or three years. I lately witnessed an attack of strangulation in a lady of my acquaintance. She suffered no person to touch her, but aimed solely at collecting, by little and little, a full inspiration, which, when accomplished, she forcibly threw off, or expended by, apparently, a voluntary effort. After a second inspiration acquired and disposed of in this way, she was much less distressed, and soon became perfectly easy and tranquil. She told me that she had often suffered similar at-

tacks, and in in each of them aimed at the same means of obtaining relief*.

To return to the case of Mr. C. Attempts to resuscitate the body were not deemed advisable, as well on account of our uncertainty in respect to the precise cause of the death, as of the very little prospect of success which attempts of this kind afford in cases where life has been suspended for so great a length of time. I opened the body, with a view to obtain better information as to the cause of the occurrence, than opinion or conjecture alone could afford me. The circumstances admitted of but a hasty and imperfect search, in which I was not able to discover any mark of recent disease, save a slight effusion of blood into the cellular substance of the lungs. The lungs, which were the only organs particularly inspected, adhered, throughout their whole anterior and lateral surface, as firmly to the walls of the thorax, as the skin does to the muscles in a healthy state of the body, by means of the cellular membrane. Traces of the mediastinum were with some difficulty recognized. This partition had acquired a texture nearly resembling the substance of the lungs; and, I have no doubt, performed, in some degree, the function thereof. The right and left lung, with their respective lobes, had ce-

* Some years ago, in making a trifling experiment with crude mercury, I carelessly threw a quantity of the metal into my mouth, and felt a portion of it rush through the glottis; a strong inclination to cough was the immediate consequence: I could, however, nearly restrain the convulsive efforts of the muscles. I quickly inverted the natural position of my body, and requested a gentleman present to jolt me up and down. Two or three globules of mercury, each the size of a pigeon-shot, directly fell into my mouth; and, on assisting the jolting by forcible expirations, several smaller globules followed, making in all about a scruple. I felt no inconvenience afterwards from the accident.

mented as it were into a single mass of lung; the pericardium had either adhered to the concave surface of this mass, and been assimilated to the nature and appearance of the same, or had been obliterated by some other process, for there was no such sac or membrane to be found; the heart was imbedded in the inferior posterior portion of this mass, and almost covered by it even in its flaccid state. The diaphragm was free from any adhesions, as was also the great curve of the aorta. I could perceive nothing that resembled tubercles in the substance of the lungs.

Some days after the examination of these organs, I mentioned the singularity in their appearance to an acquaintance of the deceased. He informed me, that, about the close of the American revolutionary war, Mr. C. had laboured under a very advanced stage of pulmonary consumption for a long time; that several of his friends, on visiting him in that state, would take, as they thought, a final leave on parting; that he recovered slowly, but under what particular treatment, or whether he had medical assistance, this gentleman could not recollect.

I am happy in being able to furnish an instance wherein a long series of good health succeeded to a very diseased state of the lungs; and most probably that form of disease termed consumption. Some physicians there are who strenuously adhere to the opinion that consumption is an incurable disease. How a mind keeping pace with the progress which medicine has made since our own time can seriously admit such an opinion, I am unable to conceive; but it is to be hoped that more demonstrative evidence than heretofore, or more correct modes of thinking, will remove from such minds an error unpardonable in a physician, and hostile to the lives and happiness of many individuals of the human family.

Account of the Failure of the Geranium Maculatum, in stopping Hæmorrhage. Communicated by William Baldwin, *M. D. of Wilmington, in the State of Delaware.*

J. H. a young man of this borough, punctured the radial artery at the wrist, with the sharp corner of a currier's knife, on the 25th of August. The hæmorrhage was profuse. Compression was employed till the night of the 30th, at which time the blood flowed as profusely as ever. The root of the geranium maculatum, recently dried and powdered, mixed with a little cold water, was now plentifully applied, and secured with a compress and roller. Appearances were favourable till the night of the 5th of September, when an alarming hæmorrhage again took place. The geranium was again resorted to, but on the following day the recurrence of a third bleeding rendered the operation indispensible, and the artery was secured with a ligature.

Whether the root, in a dried state, is less powerfully astringent than when fresh, as employed in the cases reported by *Dr. Mease*, in the third volume of the *Museum*, it is of importance to determine. To the taste it appears equally astringent, and the form of powder would seem to be better adapted to act with advantage, than the root simply bruised.

MEDICAL MUSEUM.

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Observations on the Yellow Fever. By John Stevens, *Esq. of Hoboken, New-Jersey.*

(Continued from page 83.)

IN my investigations on this very interesting subject, I have travelled forward, for a considerable part of the way, in the same road with my opponents. The advocates for domestic origin, all except Mr. Webster, agree with me that yellow fever is not specifically infectious, and thence infer that it is incapable of being imported. But the fact of its having, in numerous instances, been imported, is incontestable. Mr. Webster, in his history, has recorded various instances where the proofs of its introduction from foreign sources are unequivocal. He acknowledges, without the least hesitation, that the disease was imported from Philadelphia, in the year 1798, into Wilmington, Marcus-Hook, Chester, Newcastle, Duck-creek, &c. He tells us that "this pestilential fever carried off fifty-seven persons in the village of Marcus-Hook, where the first persons seized were a shallop-man, and others from Philadelphia. But many cases occurred which could be traced to no infection. See Dr. Sayre's letter in Currie's Memoirs, p. 136."

In the enumeration of particular instances given above of importation, I have confined myself to those only where the facts are not disputed. A single instance, indeed, of its introduction into a place from a foreign source is amply sufficient to overturn the general position of the disease being incapable of importation because it is not specifically infectious. True philosophy will not tolerate the assumption of a general principle in direct contravention of an established fact. In the above instances, the fact of the disease being imported and propagated is admited. But if yellow fever is not specifically infectious, how is this phenomenon to be accounted for? I hope I shall not incur the imputation of arrogance in attempting to establish a theory, which, in my humble opinion, furnishes a complete and satisfactory solution of the problem. I have already, I trust, proved sufficiently that yellow fever belongs to a numerous family of diseases, which are the legitimate offspring of noxious combinations taking place in the atmosphere, and hence I have ventured to denominate them *atmospheric* infections.

It is a well-known fact, that all vegetable and animal substances, in favourable circumstances, undergo dissolution, and the materials re-uniting form new compounds. To this very extensive process in nature, chemists have given the general term fermentation. This process has again been subdivided into vinous, acetous, and putrefactive fermentations. To the maintenance of this process, throughout its different stages, a due proportion of air, water, and heat appear to be essentially necessary. The combinations thus formed on a great scale in nature's work-shops are infinite. There is one curious fact, however, attending this fermentative process, on which the theory I am now about to propose is dependent. It is a subject of familiar occurrence and observation, that to commence

this process a mere assemblage of the materials, in due proportions and in all other respects duly circumstanced, is not, in many cases, alone sufficient. To insure success the addition of a certain portion of materials, in which a process of fermentation of the peculiar nature we wish to promote is actually in operation, appears to be necessary. Here, then, the whole mystery of atmospheric infections is, I apprehend, at once developed. We are now possessed of the master key wherewith to unlock all the secret recesses, in which the arcana of infections have hitherto been deposited. The familiar process, of which house-wives are in daily use, will afford a satisfactory explanation of the manner in which atmospheric infections extend their sphere of action. There cannot, I think, remain a shadow of doubt, that all atmospheric infections are the result of fermentative processes. The modus operandi and the laws by which fermentative processes are regulated apply strictly to the generation of atmospheric infections. "A little leaven leaveneth a whole lump." A single spoiled herring will in a short time communicate its taint to a whole cask of sound ones: and thus, too, by a tainted ferment, have eggs been successfully inoculated.

But to illustrate this doctrine, and to apply it more immediately to the subject matter of this essay, I will introduce a remarkable instance, among many others which might be mentioned, to prove that excretory matter, imbibed by articles of clothing operates as a ferment to communicate the infection of yellow fever. In the year ——, Gardiner Baker, keeper of the museum in New York, died of the yellow fever, and his wife removed into some interior part of New England. On opening a trunk or box containing his wearing apparel, a twelvemonth after, she was overpowered by a pestiferous

smell which issued from it. In a few days after, she sickened and died, with every symptom of yellow fever.

This single case is alone sufficient to establish beyond contradiction the theory I now advance, that atmospheric infections are the result of fermentative processes. The excretory and perspirable matter exuding from the pores of the body of a diseased person, are incapable of communicating the infection of yellow fever, plague, jail fever, &c. But place this matter in circumstances favourable to the commencement and developement of a fermentative process, and a noxious compound is soon produced, which does not fail to communicate its pestilential virus to the constitution.

Mr. Webster treats as a vulgar prejudice the idea of a " single lock of cotton enkindling a flame of disease that shall desolate a city." " It is against reason, it is impossible that an affected person or two from a ship, should, within a day or two, contaminate the air of a city, and spread the principle of disease over half a mile or a mile in extent: which must be the fact if that principle is introduced from abroad."

To those who are the least conversant with chemistry, it will not be difficult to conceive how a small portion of ferment may, under circumstances favourable to the communication and reception of this specific taint, spread infection rapidly over a wide extent. For, let us suppose the atmosphere to be fully saturated with filthy exhalations arising from all kinds of vegetable and animal substances, and under the most favourable circumstances, with respect to heat and moisture, to this mass of materials the pestilential ferment is added, is Mr. Webster competent to calculate the precise velocity with which this active agent operates? In short, if the principle is admitted,

there will be no difficulty in accounting for the rapidity with which the infection of yellow fever is disseminated. When Mr. Webster condemns as a vulgar prejudice the idea of a "single lock of cotton enkindling a flame of disease that shall desolate a city," he does not sufficiently advert to the amazing tenuity of which matter is capable. Thus, for instance, a farthing candle will disseminate rays of light to the extent of a mile at least in all directions, which rays falling at this distance on the retina of the eye will affect the optic nerve so as to cause a sensation and perception of the object. Now, were it indeed practicable, to compare the quantity of matter producing this sensible effect at this great distance, with the quantity of infectious matter contained in the lock of cotton, there is no question that the preponderance would be vastly in favour of the lock of cotton. But, to illustrate this in a manner more analogous to our present subject, we will suppose this lock of cotton to be completely saturated with the infection of small-pox. It is well known that the smallest fibre is sufficient to communicate the disease. I ask, then, how many persons would this lock be capable of infecting? But here we have the matter of infection in a tangible, visible form, as it were in gross. But, how vastly more attenuated would it be in a gaseous form! In short, if the principle I am now endeavouring to establish, that infection may be communicated to the atmosphere by pestilential ferments, be once admitted, we shall be at no loss to account for this wonderful phenomenon of a whole city being infected by a single lock of cotton. Indeed, from the almost infinite divisibility of which matter is capable, we might reasonably expect still more extensive effects.

A single subject in the confluent small-pox would probably furnish matter sufficient to infect millions. Here, however,

the ferment exerts its energies within an organized system of animated matter. But, why should not matter of an equally pestilential nature, be equally productive when its energies are exerted on animal and vegetable substances, existing in a gaseous state in the atmosphere?

Indeed, specific and atmospheric infections appear to be governed by similar laws in respect to their generation and propagation. Thus, in the small pox, for instance, which is a specific infection, the most minute particle of infectious matter, when communicated from a diseased to a healthy person, will, in the body of the latter, provided it be in a situation favourable to the process, generate and multiply similar particles to an astonishing extent: and thus, too, in the case of yellow fever, which is an atmospheric infection, the minutest particle of infectious matter, when communicated to an atmosphere favourable to the process, will generate and multiply similar particles to a like extent. The only difference then, in this respect, between these species of infection, would seem to consist in this single circumstance: that specific infection is generated and multiplied in the fluids circulating in an organized system of matter; whereas, in atmospheric infections, this process is conducted in the fluids circulating in the atmosphere itself.

But, in the case above recited of Mrs. Baker, it may be said, that although it should be admitted that the disease was contracted from the articles of clothing contained in the trunk, yet it affords no evidence of pestilential ferment communicating its taint to the atmosphere. But if this case, selected from a great number of others of a similar nature which might be adduced, establishes beyond all controversy, the fact of yellow fever being communicated by infected articles of clothing, &c.,

and if, at the same time, we deny the existence of specific infection, we are compelled, without reserve, to give an unqualified assent to this conclusion: that the pestilential virus in these and similar instances was the result of fermentative processes. But from the known laws by which these processes are governed, we may safely infer, that a pestilential taint is communicable to an atmosphere duly prepared for its reception and propagation. Indeed, the history of the introduction and propagation of the disease, in the towns and villages around the city of Philadelphia, as stated above, afford irresistible evidence of the truth and justness of this inference.

But there remains one case not yet brought forward, which furnishes direct and positive evidence, establishing, so clearly, the doctrine now advanced of pestilential ferment communicating its taint to the atmosphere, that no candid non-contagionist (as usually but improperly called) can refuse his assent.

As I feel the fullest confidence that the evidence furnished by this single case must prove decisive, and determine for ever all controversy on this very interesting subject, I shall be pardoned for going into a more minute detail than I could have wished. That I may avoid even the slightest imputation of partiality, I shall exhibit Mr. Webster's statement of facts verbatim.

"The sloop Iris, capt. Truman, from Martinico, arrived (not first at Newhaven, but) at New York, the latter part of May, 1794. Her crew and passengers being in health, she was admitted as a clean vessel, and hauled up near or at a wharf. Here she lay wind bound from eight to twelve days; the precise time is not known, but it was during a long easterly storm, which clouded the sun for nine days. While at that place,

her cargo, consisting of a few salted hides, was taken out and put on board a packet, or coasting vessel, to be transported to Newhaven. During all this time, no person on board was ill; none of the vast numbers that visited the sloop received any infection, not even the persons employed in removing the hides, and transporting them to Newhaven. Early in June, but the day is not known, the sloop arrived at Newhaven, and hauled up at the end of Mr. Austin's store, within three or four rods of the back part of Isaac Gorham's house. Within a few days after which, Gorham's wife was taken ill; viz. on the 10th June, and she died on the 15th. This was the first case of the fever. Polly Gorham, who had lived in the same house about a week, sickened on the 12th, and died on the 17th, at her father's house, three quarters of a mile distant. Mr. Austin and his clerk were taken ill about the same time, and died on the 20th: Mr. Austin at New York, and his clerk at Derby, where they went on business."

To this statement I will add an account which Mr. Webster tells us is given by a respectable gentleman of Newhaven, in the following words: " On examination, it appeared, that in the beginning of June capt. Truman arrived from Martinico, in a sloop that was infected with the contagion of the yellow fever; that this vessel lay at the wharf, within a few rods of Isaac Gorham's house; that she had on board a chest of clothes which had belonged to a mariner who died of the yellow fever, and that this chest was carried into Mr. Austin's store, and opened in the presence of capt. Truman, Mr. Austin, Henry Hubbard, and Polly Gorham; the three last mentioned of whom died in a short time after their exposure to the contents of the chest. Hence it is highly probable that Mrs. Gorham caught the disease from the infected sloop or clothing. Mr. Austin's store stands within 3 or 4 rods of Isaac Gorham's

house; and no person in town was known to have the yellow fever previous to capt. Truman's arrival."

I will also add an extract of a letter written by an eminent physician and practitioner in Newhaven. "As to the supposition," says he, "with respect to local causes originating the disease, I conceive there is no occasion to seek for any other than what was contained in the chest, which was a blanket and clothing taken off the corpse of one who had died of the fever in the West Indies, and, without the least formality of cleaning, put down into a close chest, and brought to Newhaven, and lodged in Austin's store."

The facts contained in the above statements, if duly considered, without prejudice or partiality, must furnish evidence to the mind of every non-contagionist possessed of candour, amply sufficient to convince him that a pestilential taint was communicated to the atmosphere of Newhaven, by the contents of the chest above mentioned.

Without the least intimations given to the citizens by those monitory precursors, which Mr. Webster has been at so much pains to point out to us; without any previous warnings or presages, pestilence, in its most horrific form, at once presents itself, and deals destruction around. What are the thoughts which would naturally present themselves on such an occasion? It is quite inconceivable, that a disease so different in every respect from any thing that had ever happened before, could possibly be considered as aboriginal or indigenous. Mr. Webster satisfies himself, however, on this occasion, with this single remark, that "the general state of the air in the city must have been adequate to the production of the disease." But is it not truly wonderful, that this "general state

of the air" should, in this single instance, have produced an exotic which never has appeared in this place, either before or since? If we are to look to "the general state of the air" for the prevalence of yellow fever in our sea-port towns for these ten or twelve years past, how has it happened that since 1794 Newhaven has so miraculously escaped? Would not a "general state of the air," capable of producing this disease then, have been capable of producing a like effect in this city since that time, as well as in other cities? It cannot be said, that the effects of this "general state of the air" were, upon this occasion, too weak to warrant an expectation of a recurrence; for this assertion would not comport in the least with the fact. We are informed, from the most respectable authority, that the mortality in Newhaven, in proportion to its population, was fully equal to what has occurred in any other place within the union.

Since the first settlement of this country by Europeans to the present moment, Newhaven has never been visited by yellow fever, except in the instance now mentioned. In this solitary instance how is the occurrence of this disease to be satisfactorily accounted for? To have recourse to a "general state of the air," which manifested its effects nowhere else, and in this place, on no other occasion, is egregious trifling.

But, as if apprehensive that this general cause should not answer his purpose, Mr. Webster has brought forward a specific one. He informs us that there was an accumulation of stinking fish at or near the wharf the vessel lay at. But independently of the general answer, which cannot perhaps be repeated too often, viz. that mere animal and vegetable putrefactions cannot be resorted to as adequate causes of yellow fever, I would beg leave to ask how it happens that stinking fish

should, in this single instance, not only generate a new disease in the immediate vicinity of this wharf, but should be also capable of communicating a pestilential taint to the atmosphere for a considerable distance all around? For, if stinking fish alone can generate yellow fever, and spread wide its infection on all sides, with what consistency can we deny a similar effect to the contents of the chest above mentioned?

Out of a vessel arriving from the West Indies, a chest is deposited in the store of Mr. Austin, containing the clothes and blanket of a seaman who had died there of yellow fever; of four persons present at the opening of this chest, three are taken ill of the disease and die; and, in a short time after, the disease becomes epidemic throughout the city. No man, whose mind had not been previously warped by prejudice, would hesitate a moment, taking all circumstances into consideration, to acknowledge it as his opinion, that this disease originated from these infected clothes.

That it did not originate in any local cause, one circumstance attending the rise and progress of the disease in this instance, is in my mind conclusive. The four first cases that occurred were by far the most aggravated and malignant, as was manifested by the death of all the subjects of them. Now, had the disease derived its origin from any local cause or causes, would not this have been indicated by slow and gradual approaches? Would not this higher grade of bilious fever have been preceded first by intermitting fever, succeeded by remitting fever and the ordinary bilious fever, and then by sporadic cases of yellow fever? The advocates of domestic origin, if they do not abandon entirely all attention to cause and effect, are, from the nature of their doctrine, compelled to acknowledge this to be the natural progress of the disease. But what

renders the circumstances attending this instance still more striking, is, that after the first few cases, evidently resulting from the original virus, the disease seems to have disappeared, and, when it again revived, assumed a milder aspect.

There is no possible way of accounting for these circumstances, but by the admission of the real fact, that the original virus by which the disease was introduced, was much more malignant than the taint which was hence communicated to the atmosphere, and produced the epidemic.

In this instance, I conceive, the advocates of domestic origin are completely foiled. No dexterity or ingenuity, on their part, can enable them to parry the deadly blow now aimed at their favourite doctrine. To resort to "the general state of the air," or to putrid fish, will avail them nothing. The causes assigned are in no degree adequate to the effects.

History of a Case of Hydrocephalus, and its Favourable Termination. By Dr. John Stevenson.

Newtown, Worcester County, Maryland,
April 18*th*, 1809.

Sir,

As you were pleased to remind me, in a note on my last communication, that "success, in a work of this nature, requires the joint operation of subscription and communication;" I now inform you, in order to convince you of the sincerity of "my wishing it success," that I have become a subscriber some time since; and now send you the following for publica-

tion, if you judge it worthy a place ; together with a promise of communicating whatever falls within the small circle of my observation, that I deem interesting to the medical world.

I am, Sir,

Your friend, unfeignedly,

JOHN STEVENSON.

Dr. J. R. Coxe.

ELIZABETH L———, aged nearly two years, was seized, on the 7th of August, 1808, with the usual symptoms of cholera infantum. Nothing uncommon appeared for the first day, and the mother being sick at the same time, I was called at 11 o'clock on the morning of the 8th. After prescribing for the mother, I was informed of the child's illness. On examination, it appeared to have but little fever, and gave me a pleasant smile when I uncovered its face. The family informed me it had been extremely ill the preceding day and night, with a vomiting and purging, but had been better all the morning. I observed, that as the mother had just taken an emetic, she would necessarily require all the attention the family, at that time, could bestow; and as the child was better, I should defer giving it any medicine until my return, which would be in about two hours.

About an hour after my departure, an exacerbation of fever came on, and the child had violent convulsions.

On my return, I found the room crowded with attendants, who, seemingly in a fit of desperation, had given a dose of laudanum, and immersed the child in warm water.

The fits were in a measure relieved, though they continued, lightly, through the afternoon, with longer intervals, and some permanent stiffness of all the extremities.

I was apprehensive the fits were occasioned by some acrimony in the primæ viæ that remained undischarged, and gave as a purge 3 grs. of calomel*, mixed in syrup, and directed a large tea-spoon full of ol. ricin. to be taken every two hours, until it should purge freely. Shortly after night, the fever seemed to increase, and, together with it, the convulsions returned at shorter intervals, and, lasted longer; the abdomen hard and tumefied, and no evacuation had taken place from the bowels. I grew extremely solicitous for the fate of my patient, and now made an attempt, and succeeded, in obtaining about five ounces of blood from the arm. A strong stimulating injection was thrown in the rectum, which was shortly returned without any fæces; at length, however, a stool was procured, followed by two more in the course of the night, and considerable relief was obtained. There still remained some stiffness and a spasmodic contraction of the arms, so that it was seldom any accurate knowledge could be obtained of the pulse by feeling it at the wrist; I was, however, generally satisfied when I examined the feet.

The child remained tolerably quiet the remaining part of the night. Epispastics were applied to the extremities, which, by the morning of the 9th, were well drawn.

* I had lately been led to adopt an opinion, from actual observation, that calomel, in an *appropriate dose*, would operate more speedily as a cathartic than in a *large one*. I mentioned the circumstance to a medical friend, thinking I had discovered something not generally known. He informed me he had long ago observed the same.

On my visit this morning, the dilated pupils of her eyes were the first thing that I noticed. I immediately mistrusted a diseased state of the brain; and, in order to produce a derivation from the head, gave 2 grs. cal. and 5 of jalap, to be repeated every four hours until it should produce copious evacuations from the bowels; but so insensible were the intestines to the impression of the medicine, that she relapsed into fits the succeeding night, more violent than ever, before a single evacuation was obtained from the medicine. Recourse was again had to ol. ricin. and strong enemata, which again relieved her, after being, apparently, in the act of dissolution, with every muscle composing her little system most tremendously agitated.

The bowels, after this, were not so obstinately constipated; cathartics now operated in their usual dose, and were freely used; and blisters were applied, alternately, to nearly all parts of the head. The fever was now completely subdued, but the pupils still remain dilated; no strabismus has at any time been observed; she has spoken but once since she first had fits, and that apparently in a great fright: she called her papa who was near her. She continued to lie in a torpid, lethargic, insensible state, for near three weeks, with an incessant desultory motion of her arms, unaccompanied, I am free to say, with any consciousness whatever; for the MIND, strictly speaking, *was entirely obliterated.* The loudest noise would excite no impression; the handsomest toy would not produce a single effort to handle, or even look at it.

I felt rather in an awkward predicament; I never saw a case similar to the present. She was by this time much reduced; had been weaned from the breast a short time before taken down, and again put to it; would at times suck extremely

hearty; appeared to be unconscious either of the quantity taken, or its feelings of hunger; would appear equally uneasy while at the breast as any other time. In this dilemma, nothing appeared better to be done than an adherence to the golden rule, of "prescribing according to the state of the system." Accordingly tonics were most plainly indicated, and nothing seemed to promise so much as the cold-bath. I never had tried it, in a single case, therefore felt a diffidence. Electricity was out of our reach; neither did it appear so flattering. At length, after some days spent in deliberation, recourse was had to the advice of Dr. S. Ker, of the adjoining county, who recommended the bath in such terms, that it was begun with the very next day, being the 6th of September, and continued about four weeks, regularly once a day, with the most astonishing and encouraging effects.

The bath was composed of a large tub, set in the room, filled with water from the well; for the first few times made a little warmer. The child was entirely stripped, simply plunged, immediately received in a warm blanket, rubbed well with dry flannel, and wrapped warm.

It was highly pleasing to see the resuscitation of its mental faculties, and the gradual manner in which they were re-evolved. The bath was used about eight days before any effect was perceptible. The first effect observed was (what would most naturally be expected) signs of uneasiness on being stripped; some days afterwards she would look at the tub before being stripped. She, after a while, would follow with her eyes the other children about the room; and a while, still longer, afterwards, began to attempt to support herself on her feet (for I ought to have observed before, that her feet have been as useless as her organs of speech, and have

been idle as long), and finally she made an effort to speak, for the first time, wanting two days of seven weeks.

The exhibition of the martial tincture was begun at the same time the bath was used; but, having some constipating effect on the bowels, and the appetite sufficiently good, it was not long continued. The bath was intended to have been continued somewhat longer, but an eruption (the psora) appeared generally over the skin, with such intolerable itching, that the bath was laid aside for frictions of sulphur. The child, however, continued to mend rapidly without any further use of it.

I have not observed her speech, or the use of her lower extremities, to improve any faster, in consequence of her having had a previous use of them, than is customary in other children.

She is now, though rather small of her age, in perfect health, lively, and active.

Experiments and Observations on the raising of Wheat Flour, and Buckwheat Meal. By JAMES WOODHOUSE, *M. D. Professor of Chemistry in the University of Pennsylvania, &c.*

AN opinion has been adopted by many philosophers in Europe and America, that the raising of bread is merely owing to a discharge of carbonic acid gas or fixed air; and it has been asserted, that there is no difference between the properties of flour, and bread when it has been baked.

This opinion was supported by the late Dr. John Penington, in a volume of Chemical and Economical Essays, in the year 1790; and, since this work has been published, this theory has generally prevailed in the United States. The editors of the Encyclopædia Britannica brought forward what they supposed to be a few facts in support of it, and they supposed that common atmospheric air, entangled in the midst of dough, would be sufficient to raise it.

The principal argument in support of this opinion is, that all waters which contain carbonic acid, such as those of the Saratoga and Ballstown springs, in the state of New York, easily raise bread.

It is not my intention to prove that the raising of dough is not owing to an escape of fixed air, but to show,

First, that flour mixed with water, however strongly impregnated with carbonic acid, will not make light bread.

Secondly, that the best bread can be made without any fixed air; and

Thirdly, that a chemical change takes place in the component parts of flour when it is made into bread, and which arises from the decomposition of water.

A quantity of water, impregnated with carbonic acid, by strong compression, was accurately mixed with buckwheat meal, and exposed to a proper temperature. It exhibited no signs of rising; the meal subsided into a heavy mass, and the water floated over its surface.

A quantity of yeast was filtered. A viscid residuum remained on the filter, which contained no carbonic acid.

Half a tea-spoon full of this substance, triturated with warm water, and well mixed with buckwheat meal, raised it completely in four hours.

Dough was made from wheat flour, by well mixing it with water containing carbonic acid.

It was also made with the viscid residuum and water.

The masses were placed along side of each other by the fire, and, in the space of a few hours, that made with the residuum was well raised, but the mass from the carbonic acid exhibited no change; and when the two were baked, the loaf made without the fixed air was extremely light and spongy, whereas the other was tough and heavy.

These experiments clearly prove, that buckwheat meal may be raised, and excellent wheat bread made, without any fixed air; and that water, however strongly impregnated with carbonic acid, and mixed with flour, will not afford good bread.

The bakers of Paris make this article of an excellent quality, yet all their yeast is brought in bags, in a dry state, from Flanders and Picardy.

Good yeast contains a large quantity of fixed air; but so little of it is used in raising buckwheat meal and wheat dough, that it cannot have much effect in raising these substances.

By boiling forty cubic inches of yeast in a glass retort, the mouth of which communicated with a receiver filled with mercury, fifty-two cubic inches of fixed air were obtained.

A small loaf of bread was raised with the viscid residuum, in one hundred and four cubic inches of atmospheric air, confined by water. In the space of sixteen hours, the air was found to contain forty cubic inches of carbonic acid gas. Upon washing away the fixed air, and testing the atmospheric air by phosphorus, it was found to have undergone no alteration.

Buckwheat meal was raised in the same manner, and with the like effect.

These experiments prove, that a chemical change takes place in the component parts of flour, when it is made into bread. No carbonic acid was contained in the dough when it was first made, and yet, in a short time, forty cubic inches of this gas was formed.

Fixed air is composed of carbon and oxygen.

As no oxygen is contained in the flour, and no carbon in the water, the oxygen of the water must combine with the carbon of the flour, and generate the air which raises the bread.

It is a difficult matter to say what becomes of the hydrogen of the decomposed water. It does not unite with another portion of oxygen and carbon to form alkohol, for none of this fluid can be procured by distilling dough or buckwheat meal, after they have been raised.

An Essay on the Treatment of Oblique Fractures of the Os Femoris. Read for junior membership, by EDWARD M. FOULKES, *before the Philadelphia Medical Society, January,* 1809.

SIR,

AFTER the repeated solicitations of many of my acquaintances for the publication of the following essay, I have at length consented to submit it to your judgment; and if you think it worthy of promulgation, you are at liberty to insert it in your useful Museum.

I am, dear Sir,

With sentiments of the highest esteem,

Your humble servant,

EDWD. M. FOULKES.

Dr. J. R. Coxe.

MAN, from the love of fame, or from motives purely charitable, has been busily engaged, for centuries past, in inventing, devising, and executing means for the relief of his suffering kind. To prove this, it is necessary only to advert but for a moment to the records of the science of medicine. Contained there, we shall find an account of the successive improvements in that science, not less astonishing than pleasing. It is astonishing, because of the discovery of the wonderfully seeking, and in most instances *finding*, ingenuity of man. It is pleasing, because of the discovery of the means being efficient in the relief of suffering mankind.

When charitable motives give rise to such attempts, men, so far from becoming the objects of derision and ridicule (as

ought to be the case when caused by a desire of fame) are, in the estimation of the learned, the objects of incalculable affection.

I then shall offer, as an apology for the following innovation, the action of these motives.

It is a subject of no little surprise, that, considering the length of time which has elapsed since the earliest period at which surgery was practised, and the many accidents and diseases to which our ancestors were exposed, during this time, belonging to the province of surgery, there should be so many desiderata in this branch of the healing art. Thus, in oblique fractures of the os femoris, it was reserved till the late period at which Dessault lived, to discover the method of obviating (by means of permanent extension) that, that the more ancient surgeons thought to be a necessary consequence of such affections. The reader will readily perceive, that I mean deformity and shortening of the limb. And these are, even now, not prevented but at the expence of great suffering to the patient, notwithstanding the improvements made by doctors Physick and Hutchinson upon the treatment recommended by the French surgeon.

Before proceeding farther in this essay, it will be proper to mention more particularly why it was undertaken, and the order to be observed in it. It was undertaken, in common with all other means, because of the hazardous, helpless, and agonizing state of persons labouring under those affections, before means for their relief were had recourse to. Secondly, because of a knowledge of the sufferings of patients while under both the ancient and modern treatments. And, thirdly, from seeing frequent failures in the cure as attempted by those means. The

first thing, then, which shall occupy a part of this essay, will be a mention of the objections to the most successful ancient and modern treatments: the second, a statement of the indications of cure: the third, a description of the means and the method of applying them, by which I think the indications will be fulfilled: the fourth, an inquiry to ascertain whether or not they are: the fifth, an attempt to do away all objections to it: and the sixth, a brief detail of its advantages.

The objections to Dessault's apparatus for making permanent extension are, first, the liability of the parts on which the roller for making counter-extension acts to be excoriated, by the roller becoming somewhat like a rope, and consequently bearing on but few points with great force.

Secondly, by the oblique passage of the roller over the superior part of the thigh, the muscles on the internal side are irritated, and in consequence contract, producing thereby derangement both in the length and direction of the thigh; in which last it is assisted by its own oblique pressure on the superior fragment, tending to carry it upward and outward.

The muscles most exposed to the irritation, and by whose contraction this effect is produced, are, the adductor magnus femoris gracilis, or rectus internus, sartorius, semi-tendinosus, and semi-membranosus.

For these muscles, originating from the ramus pubis, and tuberosity of the ischium, and inserted into the inner and inferior part of the femur, and into the inner and superior part of the tibia, their action certainly would have the effect of drawing these parts upward and inward; and in proportion as they were drawn in this latter direction, would be the displacement

of the fractured portions with regard to the direction of the thigh. For the muscles which are inserted into the lower end of the superior fragment are so deep seated as to be without the reach of the irritating power of the roller, and of course do not contract so as to keep that end in contact with the other, and farther prevent its being carried outward by the pressure of the roller. It would then form an angle with the superior end of the inferior fragment, whose point would be outward, and the angle looking inward, so than the knee would be much nearer the sound leg than would be consistent with proper walking and the good figure of the person.

This second objection applies with much more force in fractures of the neck of the bone, than of its body.

In the modification of the external splint of Dessault, by Dr. Physick, these objections apply with much less force. For, as regards the first, the danger of excoriation in the perineum is lessened in proportion as the pressure on it is; which is done by dividing it between the perineum and axilla; and also in proportion as the roller is carried from an oblique situation, to one more in the direction of the thigh.

And as regards the second objection, both the danger of carrying the superior fragment outward, by the pressure of the counter extending roller, and the irritation of the muscles by its oblique action, are much lessened by the passage of the roller in a direction approaching nearer that of the thigh. Nay, the doctor even tells us how we may make it act precisely in this direction.

But as I shall presently say this method is likely not to be employed, because of the ill conveniences attending it, doing away the propriety of its application.

I come next to point out the objections to the doctor's manner of treatment. They are, first, as in Dessault's method, an inefficiency in preventing a shortening of the limb and deformity in its direction, produced by the action of the counter-extending roller irritating the muscles, and carrying the superior fragment outward, as was explained when speaking of this as applying to Dessault's apparatus.

But, say the votaries of Dr. Physick, we are taught to avoid this, by tying a bandage to the counter-extending roller on its anterior part, and passing it around the unaffected side of the body, and tying it tight to the posterior half of the aforesaid roller; by which we pull it from the affected side, in a line running perpendicularly from the tuberosity of the ischium over the groin, and up about an inch and a half distant from and parallel to the linea alba. This brings me to the consideration of the second objection, and which I formerly hinted at: viz. the inconveniences of the continued application of the transverse roller; or that which is used to pull the counter-extending roller in the aforementioned position.

It must be obvious to all persons, that the force necessary to be employed for this purpose must be very considerable, and, to do this, it acts on the unaffected side of the thorax; from which three ill consequences are apt to arise. Before proceeding to mention these, I beg leave to observe once for all, lest, from a neglect of it, I may not be perfectly intelligible, that when I speak of the unaffected side of the thorax, the reader is to understand that side which is opposed to the fractured thigh, and the reverse when speaking of the affected side, that is, the side corresponding to the diseased thigh.

The bad effects of this roller are, first, a difficulty of confining it in a horizontal situation, and which is necessary; for if the part of the roller passing on the unaffected side of the thorax be left unconfined, it will either ascend or descend, because of the conic figure, and motions of the chest by respiration.

Now, if that part of the roller descends, it will pull its ends down, which, approaching to the unaffected side, by reason of the obliquity of the counter-extending roller, will entirely destroy its action: for it is in this situation perfectly relaxed; and the consequence of this is, a resuming, by the counter-extending roller, its first oblique position and action.

I am aware that it will be said, in reply to this objection, that the transverse, instead of being tied to the counter-extending roller, should be sewed to it, and that by so doing the consequences of its slipping down would be avoided. But even this would be attended with hurtful consequences: for, in proportion as the roller moved from a horizontal situation to an oblique one, would be the consequent pressure, and the danger of excoriation. Nor is this all, for the ascending and descending parts of the colon, lying between the small intestines and parietes of the abdomen, would be, according as the right or left sides of the body are acted on by the descended roller, very much compressed; and consequent, and dependent on this, would be an imperfect or an entire cessation of the performance of the functions of that intestine.

Should the part of the roller lying on the unaffected side ascend, its ends, whether tied or sewed, cannot follow it: for, when tied, the counter-extending roller goes so obliquely outward from the place where the two are attached to each other,

that it does away all possibility of such an occurence ; and, if sewed, it is evident, the ends could not move.

Now the consequence of this would be, augmenting that, that before was too great, to wit, a difficulty of respiring freely, caused by pressure. For, as I before observed, in proportion as the roller moved from a horizontal to an oblique situation, would be the consequent pressure. This impediment to respiration being afforded when the roller remains in its place of first application, is the second objection to its use.

The third is, from its great pressure, it is very apt to produce excoriation of the parts on which it acts.

The next objection to Dr. Physick's apparatus is, that the external splint presses in the axilla : from which results four ill consequences, namely, in the first place, the circulation in the axillary artery is almost completely stopped, and the return of the blood is also stopped. Arising from this are, first, a numbness and disagreeable pricking in the arm, forearm, and hand ; secondly, discoloration of the afore-mentioned parts ; thirdly, coldnesss of them ; fourthly, swelling ; fifthly, vesications ; and, sixthly, mortification of the parts.

Any person may satisfy himself of the truth of the first, second, and third remarks, by sitting thirty or forty minutes in a chair whose back extends up as high as the axilla, with the arm thrown or hanging over it, in such way as to allow the weight of the arm to be supported by the bearing of the axilla on the chair. He will, as before said, first feel a numbness and pricking sensation in the depending parts, secondly, a discoloration of a bluish red will come on, and, thirdly, cold-

ness. If now the radial artery be felt, its accustomed pulsatory motion will be found to have deserted it. By a continuance in this position even one week, the accession of all the before-mentioned horrible effects will take place.

The second inconvenience is the extreme danger of producing excoriation where the splint touches the pectoralis major and latissimus dorsi.

The third is, either a painful swelling of the axillary glands, which is apt to terminate in cancer, or such a degree of inflammation as to be troublesome, and to require a pretermission of the use of the splint.

And the fourth ill effect resulting from the use of the splint is, that it pushes the shoulder so far from the spine of the ilium of the affected side, as, by putting the muscles and integuments completely to the stretch, to be productive of the severest pain and greatest distress to the patient.

I once asked the adjunct professor* of surgery in the University of Pennsylvania, whether or not he had seen the aforesaid effects accrue from Dr. Physick's method of cure? (For I had not seen a case of fracture treated by the doctor's method, and of course could not ascertain it from experience.) His answer was, that he had not. I then asked him, if he did not think their occurrence quite probable? he said no; and gave this as a reason, more, though, to account for their *non-occurrence*, than to answer my last question. It was this, that the patient might alternately support the pressure on the perineum by the counter-extending roller, and in the axilla by the splint,

* Dr. Dorsey.

and thereby avoid those bad effects. But it must be obvious, that if the patient tried to free himself from the pressure of the splint, he would augment the pain and distress already existing, in consequence of the stretching of the muscles and teguments of the side, for he would have to bend his body to the unaffected side much more than the splint could possibly do, and in proportion as this is done will be the increase of pain. So, with due deference to Dr. Dorsey, I do not think that he has satisfactorily accounted for the non-occurrence of those effects.

Then how shall we explain the fact? We can explain it by saying, that the splint does not press in the axilla so as to take off half of the counter-extending pressure from the perineum. And this I believe to be the true cause. And I farther believe, that if the splint were three inches shorter, cures would be effected with the same ease and perfection that they now are. To substantiate my belief, look at the experiment with the chair, consider the exposure of the blood-vessels, glands, &c. the tenderness of the parts, the nature and continuance of the pressure. A due consideration of these things cannot but induce a belief with me, even in the most sceptical mind.

To the foregoing objections to Dr. Physick's splint may be added this, another inconvenience, viz. that all persons not of a particular height or length must have a splint made expressly for themselves. An inconvenience this, worthy of much attention.

I hope I shall not be censured for having been thus bold in decrying a treatment whose advocates are not less respectable than numerous. For it is, I believe, uniformly the case, that whenever innovation in science is attempted, the innovator

finds it necessary to decry or expose the imperfections of the old, so that the world may see that perfection was not had; and now their wishes for it prompts them to seek it, in doing which they are almost certain to give this attempted innovation an attentive examination. Should I fail in the following attempt, I hope my errors will serve as a clue, to reason from which, some more happy genius may discover that mode of treatment which shall in every respect be complete. Nor is this improbable; for, says Rush, " New truths are seldom discovered, but at the expence of old errors."

I proceed next in order to mention the indications of cure which appear not to have been fulfilled by former apparatus.

They are, first, to make the roller which passes over the tuberosity of the ischium next the affected thigh, act in this direction of the limb.

Secondly, to prevent perfectly any pressure in the axilla.

And, thirdly, to lessen the pressure on the perineum.

It being now in order, I proceed to a description of the means by which I think they are fulfilled. In the superior end of Dr. Physick's splint, I would have the crutch-like excavation so deep as not to come in contact with the axilla, and of course, when it acts, not to press there.

To each part, A A, of the splint as they pass before and behind the joint of the arm, at their tops, is to be fixed, by an immoveable joint, a piece of wood B, of the same width of the upper end of the splint, making an angle of about 135 degrees with it, and about nine inches in length. This is also to be exca-

vated, but at its lower end, and the excavation is to be so large as not to touch the shoulder which it embraces ; its upper end may be rounded in the form of a shingle ; and it is to have on its superior surface a piece of light wood C, about an inch thick, glued ; this is to strengthen and prevent this part from splitting, for on it counter-extension is to be made. Between the edge of the excavation and the superior convex end, are to be two mortise holes D D, about an inch and a half long and half inch wide. At the immoveable joint made by the piece B and the splint, it would be well to have a piece or block of wood E, fitted and glued well, to enable the joint to support better the strain, which is considerable. The length of the splint, taken together with that of the piece of wood B, should be between six and seven feet. The splint below the middle point, between the hip and arm-pit, may be of the ordinary width, but above that it must be so wide as to admit the hole made by the two excavations to be so capacious as not to touch axilla, shoulder, nor any other part in their neighbourhood ; and also so wide as to make the branches or horns strong enough to sustain the pressure of counter-extension. It (the splint) may be of the ordinary thickness. Two leather straps F and G ; the one F should be twelve inches in length, and about an inch and a half wide, and have two common button holes cut in one end, while to the other a common buckle is to be sewed. The other strap G should be about three feet in length, and as wide as the buckle of the strap F ; this should also have, at one end, two button holes cut, and through the greatest part of its length, beginning at the end unnoticed, should have holes punched to receive the tongue of the buckle of the strap F.

A roller should be procured of about five feet in length and two or three inches in breadth. The pantaloons of the patient

(if a male), if not, the pantaloons of a person about the size of the patient, should be ripped in the seam which unites the two legs, so as quite to separate them; to the bottom of the leg of the pantaloons belonging to the unaffected thigh, should be sewed a common sock; let it be cotton or woollen, according as the season is warm or cold in which the accident happened. A pad of chaff long enough to reach from the groin to the instep. A splint about four inches shorter than the pad, and of the common width, and a common roller to confine them. These things being had, and the patient laid on a suitable place, viz. a mattress, with the body-bandage of Dessault under him, and the splint cloth, and bandage of strips placed under the affected thigh, as directed by Dr. Physick, the leg of the pantaloons, having the sock sewed to it, is to be put on the unaffected limb and pulled up well; the strap G is then to be buttoned to the posterior button of the pantaloons, to which the suspenders were wont to be, and its other end carried to the shoulder of the affected side: extension and counter-extension are now to be made, the fragments put in apposition, the bandage of strips applied, the two short pasteboard splints, which go on the anterior and posterior parts of the thigh, and the two pads of chaff, are all to be applied according to the directions of Dr. Physick, by whom they are used and recommended; this done, the splint which I have described is to be wrapped up in the splint cloth, care being taken to leave enough of it to wrap up the internal splint of Dr. Physick. After being unfolded, the arm corresponding to the affected limb must be put through the hole or opening at the top of the splint, in such way that the holes D D will be over the middle of the shoulder; the body-bandage is now to be tied around both body and splint, so as to fix this last to the former, and also prevent its bending at the joint H; the strap G is to be put through the posterior of the holes D D, when its end will be over the superior surface of the upper end of the

splint, it is then to be put through the anterior hole and carried down over the breast. The strap F is now to be buttoned to the anterior suspender button of the pantaloons, to which then the strap G is buckled, and may be drawn as tight as the surgeon pleases. After the extension has begun by the splint, the five-feet roller is to have its middle applied to the perineum of the affected side, and its two ends, one posterior and the other anterior, are to be carried up and passed through the corresponding holes D D, and then tied to each other. The extension is to be made after Dr. Physick's directions or method. The remaining part of the treatment, as the application of Dr. Physick's internal splint, the tying the bits of tape to make the whole firm and secure, is according to the doctor's method.

There is a circumstance, which, though not properly an indication in the cure, deserves the attention of the surgeon to prevent even a possibility of its occurrence, viz. that from an inclination in the patient to change some little the position of his body, derangement is produced: for to do this he must draw up the foot, and fix the leg nearly perpendicular, and then cause the muscles on the anterior parts of the body and thigh to act so as to make the foot and head be the points on which the whole rested; these therefore must be fixed, and, as being so, may be said to be the origins of the muscles concerned in producing the wished-for yet deleterious motion. I say deleterious motion, for a sort of circumrotatory motion is given to the unaffected side, whose centre corresponds to the axis of the affected thigh; now the lower fragment not partaking of the rotatory impulse, must remain steady, and the consequence is fresh derangement. Then, to avoid the danger of this, I would have the pad formerly described, placed on the anterior parts of the thigh and leg which are unaffected; over this the splint should be applied, and the whole secured by a roller.

The apparatus being described, and its manner of application, I come next to inquire whether or not the indications of cure are complied with. The first certainly is: for a line running from the tuberosity of the ischium up to the middle of the shoulder, when the body is perfectly erect, will be found to be perpendicular or nearly so to the horizon, and this is the direction of the thigh, and precisely the situation of the roller, which has for its object the prevention of the descent of that side of the pelvis corresponding to the diseased thigh, and which tendency is given to it by the action of the leather straps and the extending roller; but in doing this (which it effectually does), it also makes half of the counter-extension.

It is equally certain that the second indication is fulfilled; for it must be remembered, that, when describing the splint, it was particularly observed, that the crutch-like excavation was to be so deep and large, as not to touch the arm or axilla.

And as to the third indication, it is not less evidently fulfilled. For half the resistance necessary to be afforded to prevent the body following the lower fragment, which is continually pulled down by the extending roller, is taken from the perineum, and transferred to well and distant parts (the foot, leg, and thigh), by the splint offering a place near the unaffected side, for a counter-extending roller to act on. Now, was this attempted in other apparatus, the obliquity of the roller would be such as to produce inconveniences so great as to require a discontinuance of its use.

It is next in order to inquire whether there be any disadvantages attendant on the continued application of this apparatus, whether these are greater than those of other apparatus, and

whether they are such as do away the propriety of its application?

There is one, but this can be obviated with the greatest ease: it is, that the bulk or size of the part A, which passes behind the shoulder, is such as to support a part of the weight of the body, for it cannot lie in perfect contact with the bed near this part. The objection is done away, first, by pressing the splint directly down on the bed after it is applied, by which a depression or groove will be made for the lodgement of the part; the body will then lie on a plane whose superficies corresponds to that of the bed. Secondly, by the imposition of a flannel pad between the shoulder and splint. And thirdly, by having two splints, one for each side, these having the posterior parts cut entirely away, while the anterior are made so strong as to support all that pressure now borne by both parts.

There is one other objection, which is, that counter-extension cannot be made on both sides, when both thighs are broken. But, if my reply to Dr. Dorsey's assertion be correct, this objection is quite futile; nay, in making use of that reply in controverting this objection, it receives additional probability of correctness, from the circumstance of the counter-extending roller acting in the direction of the thigh, and thereby employing but little force to do that, that requires the greatest when it passes obliquely, as it does in Dr. Physick's method of treatment.

To exemplify my meaning, let there be a body weighing two drachms; to this, by a cord, fix another weighing eighty-six grains; place the heaviest on a well-planed table, so near its edge that the lesser may hang an inch or two over; then re-

move the hands from them, and it will be found that the lighter body will draw the heavier entirely off.

Secondly, remove the lighter body from the cord, and in the place where the heaviest lay, in the experiment just mentioned, put it again: then lay a ruler parallel to the track observed by the body in going off the table: let the ruler be thin and smooth, measure the distance at which the body is from the edge of the table, and then measure the same along the edge, beginning at the edge of the ruler, which is coincident with the scite of the cord, as in the first experiment. After getting this by measurement, place the cord there, in such way though, that it will cross the ruler; that is, the ruler must be between the body on the table and the point on the edge of the table, at which the cord is to act in pulling the body. Now suspend to the cord a weight which will draw off the body, which will require a weight equal to two hundred and twenty grains; for it must be observed, that to do this (pull it off) it acts in the diagonal of a square, one of whose sides is the distance from the edge of the table to the body, and another from the same point to the place on the table at which the cord touches in hanging down. Here then we see that the force necessary to do the same thing, when employed in the diagonal (or, in other words, when acting obliquely), is nearly thrice as great as when acting in a line parallel to the square, that is, in a straight line. Now my reply to Dr. Dorsey's assertion was, that I believed the pressure of counter-extension to be supported solely by the perineum; but, if the perineum be able to support almost thrice the pressure necessary to keep up the superior fragment, as the above experiments show to be the case, how much more easy, then, will it support the necessary pressure? no more than which would be made, even after excluding the leather straps and pantaloons; and for this plain reason, that the counter-ex-

tending roller, in the treatment I have proposed, acts precisely in the direction of the thigh.

Besides, Dessault says, that his cures were generally perfect. Now counter-extension, in his mode of treatment, is made entirely on the perineum ; and we know that his counter-extending roller acted so obliquely as to approach near a right angle ; and if he could obtain cures in this way, it would be unaccountably strange that we could not obtain them with less suffering to the patient, in less time, and with more perfection : for we proceed in the cure with means accurately adapted to the indications given by the nature of the affection.

There is another objection, which is a little danger of producing tenderness and excoriation on the heel of the unaffected side. But this is so easily avoided as scarcely to deserve time to mention the means. All that is necessary is to elevate the leg, by placing a pillow under it, taking care to prevent its lower edge extending to the heel.

After having stated the objections to the treatment I have proposed, and endeavoured to do them away, it is left for the candid reader to judge of their weight, and of my success in refuting them ; or, in other words, it is for him to determine whether or not the disadvantages attendant on the treatment, are such as to do away the propriety of its application.

It remains now for me briefly to mention the uses of some parts of the splint not yet treated of ; and then conclude, by simply stating some of its most prominent advantages.

The parts not yet spoken of are the joint H, the mortise holes I I I I, the gimlet holes K K K K, and the screws L L,

which fasten the block to the splint intended to make the extending roller act in the direction of the leg. The use of the first, namely, the flexible joint, is to make the splint portable, by allowing it to be doubled up, so that the surgeon need not be at the trouble and expence of making one in every case, and which all would have to do, except those who practise in hospitals or like institutions. This would invariably be the case with a surgeon in the country; unless, indeed, the accident should happen at his own house.

The uses of the second, third, and fourth parts, namely mortise and gimlet holes, and screws, are to adapt the point of extension in the splint to the different lengths of patients above twelve years old. For, by unscrewing the screws, the block may be moved and fixed three feet up the splint, and at any intermediate point the surgeon chooses. Now it would have been inconvenient, and perhaps detrimental, to have had a long extending roller, and, after fastening it properly to the foot, carry it down to the end of the splint, and there secure it in the ordinary way, which would have to be done whenever short subjects were submitted to its operation. But the gimlet holes allow the block to be fixed as near the foot as the surgeon wishes, by their admitting the screws in them; and the mortise holes allow of the roller being fastened to the splint as soon as it has passed over the block, so that a roller of ordinary length may be used with all its advantages. The mortise holes may be about six inches apart, but disposed in pairs, that is, directly adjacent to each other; and may be an inch and a half long, and half an inch wide. The gimlet holes should be about midway between the mortise holes, and disposed also in pairs.

From these contrivances it must be obvious that the objections made to Boyer's apparatus for treating those fractures are not applicable to this.

The objections were, that every patient not of a particular length was obliged to have another machine made. Here, on the contrary, as was before said, this, by simply moving the block, will fit all persons above twelve or fifteen years old. This is an advantage possessed, that but very few of the former apparatus can boast of. And now to conclude.

The advantages of the treatment proposed are, first, that half of counter-extension is made on a surface so extensive (the foot, leg, and thigh) as to escape entirely the notice of the patient, so that inconvenience from counter-extending pressure is not to be apprehended.

Secondly, the roller passing on the tuberosity of the ischium acts precisely in the direction of the thigh, without the aid of a transverse or any other hurting roller.

The consequence of this action is, an exemption of the muscles of the thigh from irritation, and thereby avoiding the danger of producing derangement in the lower fragment; moreover the danger of carrying the upper fragment outward by the presure of the roller is perfectly done away.

Thirdly, all the ill effects of pressure in the axilla are avoided.

Fourthly, the use of the arm corresponding to the diseased thigh is permitted to the patient; whereas in the mode of treatment recommended by Dr. Physick, if the splint presses in the axilla, he is deprived of it. If there were no other advantage

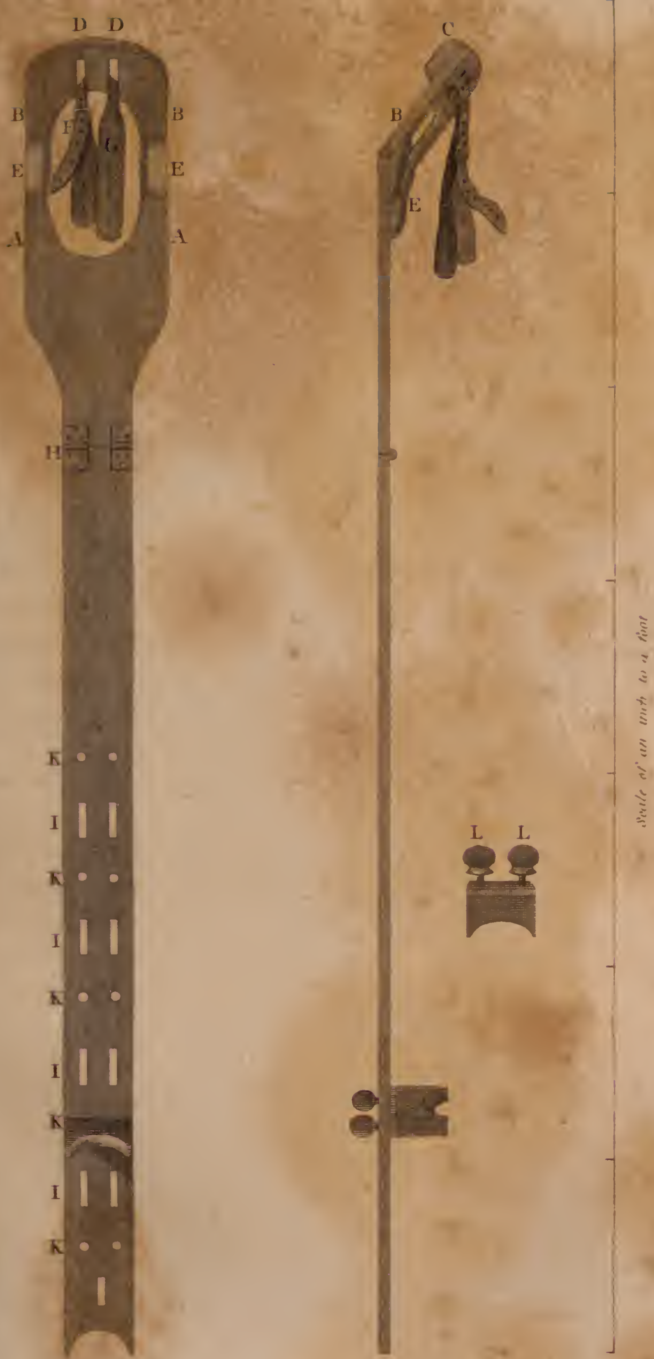
derived from the confinement of the sound leg, this would amply compensate for it. Who is it, that would not rather have the use of his arms when confined on his back, than of his legs?

Fifthly, as the counter-extension is made on both sides of the pelvis, this is kept square, whereby a very accurate opportunity is afforded for measuring the two limbs, that is, comparing their lengths with each other.

Sixthly, by its fulfilling so completely every indication of cure, there is reason to believe that it will not only make the cure more perfect, but even expedite it.

Seventhly, it may be employed with all the aforementioned advantages in oblique fractures of the bones of the leg, when, from excoriation of the leg by the counter-extending roller, the ordinary apparatus is to be discontinued.

And eighthly, because of its simple structure, any mechanic can make it, from having it described to him by the surgeon.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Scale of an inch to a foot

Account of a Fœtus found in the Body of a Child, with the Dissection. In a letter to the EDITOR, *from Dr.* EDWARD B. GAITHER.

Springfield, Washington County (Ken.), June 4th, 1809.

SIR,

AS the science of medicine is not yet consummate, and believing that nature has in reserve many secrets of great importance to the human family, which the mind of man is capable of grasping and comprehending, should accident or a chain of reasoning present them a clue to direct their inquiries, I should conceive myself unpardonable not to present to the medical world, believing the case not on record, a very singular phenomenon, which the learned may avail themselves of either to elicit or extort from nature many valuable secrets.

On the 7th of April, in the above-mentioned county, I was called to visit a female child, the daughter of John Milbourn. She was two years and nine months old, and was supposed to be affected with ascites. She died about three hours after my arrival. Her parents gave me a detailed account of her case, and its various symptoms. I was by no means satisfied that it was a real dropsy, though there was great tumefaction and tenseness of the abdomen, and fluctuations evidently felt when prest by the hand; but as the symptoms were some of them inappropriate to that disease, and others so equivocal, I could not but suspect that her disease had been either unknown or misconceived; I prevailed on her parents to permit an instrumental examination.

The operation was performed by a longitudinal incision passing from below the sternum and reaching near the pubes, and a transverse one passing through the epigastric region and sides; a cavity was opened to about half the distance between the abdominal cavity and the anterior surface, that discharged between three quarts and a gallon of yellow water, which smelled like rotten eggs. Within the cavity was found a monster, and also an animal substance of a whitish colour. The monster weighed one pound and fourteen ounces; the substance weighed two ounces, was rather of an oval figure, and connected to the child from which it was taken, by a cord that had some faint resemblance to the umbilical. On one extremity of the substance is a small teat or protuberance, about half an inch long, and between one-fourth and one-half of an inch in diameter. Immediately by it is hair of an auburn colour, about an inch and one-fourth long. The only analogy it bears to the human is, that is covered by the epidermis.

The monster occupied part of the epigastric and the umbilical regions. It was not connected to the child from which it was taken by a cord or any visible medium. Whether a cord or other medium of connection had existed and been destroyed by putrefaction (which, from the smell of the fluid and other appearances, had commenced) could not positively be ascertained. That there must have been some medium of connection I am confirmed, as well by the universal course of nature and analogy on this subject, as by an appearance at the articulation of the cervical and dorsal vertebra, resembling faintly the divided funis.

The position of the monster in its envelope was aukward; its thighs drawn up to its abdomen, and attached to it in places, the left resting on the shoulder, and reaching as far as

the back part of the head; the right resting or pressing on the back of the right hand. The ossa femoris have perforated the flesh at the knee, and are about half an inch out. The left leg is imperfect, lies back along the thigh, to which it has grown. The right leg is also imperfect; its foot is suspended over the head. On one foot are three toes; on the other a small appearance of two. From the knees to the shoulders there is considerable perfection of form. Its sex is indistinctly marked; the indications are of the feminine. The left arm should rather be called a stump than an arm; it has on it no hand; at the end of the stump is to be discovered a nail. The right arm is large and long, it has three fingers and the thumb. The head is very imperfect, it rests upon the breast between the knees. It has neither ears nor eyes, or appearance of any substitute for either. No mouth, nor any thing that has a near resemblance to it. There is on the left side of the face, or rather that region of the head which the face should occupy, a small prominency, which contains three teeth, the canine and two incisores; they are about the size of the teeth of a child two years old. This prominency, or mouth, if it may be so called, has no aperture. On the back part of the head was hair of an auburn colour, eight or nine inches long. The body of the monster was 7 inches long, and 10 in circumference. The thighs 6 long, and 8 in circumference. The arm 5 inches long. The stump not quite 4 inches in length.

The interior of the cavity which contained the monsters, resembled the membrana decidua. This appearance was assumed; for, upon examination, there was not any vestige of membrane peculiar to the monsters discovered. Having explored this cavity, and dislodged its contents, I extended the incision through the muscular partition into the abdominal ca-

vity, and examined the viscera. They were rather pale, otherwise natural.

The little girl that those monsters were taken from, for about nine months after birth, was healthy. Her parents discovered, when she was only a month or two old, something hard within the abdomen, which continued to increase. After this time she became less healthy, but her complaints were those incident to all children. About nine months prior to her death, she began to decline, and became much emaciated; her appetite continued strong; her longings and desire for ardent spirits were great; she would become intoxicated if indulged in the free use of them; it took a considerable quantity to affect her; she drank freely an hour before her death. I believe it was the use of spirits, in part, that supported her so long. She was of the ordinary size of children at her age, had dark hair and eyes, and would have been handsome but for a gloom and melancholy that sat upon her countenance, which made her appearance peculiarly interesting. I have now in my possession the monsters, and am in hopes shall be able to preserve them. Should there be any disquisitions worthy of notice, or plausible opinions, I should be happy to receive them.

EDWARD B. GAITHER.

Dr. J. R. Coxe.

☞ See an account of something similar, by Mr. Blanche; Museum, vol. II. p. 226.—EDITOR.

A case of Fistula in Ano, in an Infant of eight Months. In a Letter from Dr. FELIX PASCALIS, *of New York, Physician for the County and City Prisons, consulting Physician of the Alms-house.*

New York, June 18, 1809.

SIR,

YOU will find important circumstances in the following case. Although simple in itself, it impressed my mind with the opinion that it might be a warning against its possible formation in an infant, and that its perfect cure should encourage a speedy instrumental process.

The male infant of Mr. Stephen Rich, of this city, was taken from his mother two months before she died with *phthisis pulmonalis*. I objected to the nurse with whom he was left, because she had her own infant as another nursling. Some time elapsed before an acceptable woman could be obtained, when this child, visibly much debilitated, began to show a considerable eruptive disease; he had also a few angry boils. A large one was observed on the internal margin of the anus, which I would have deliberately opened, had I not been strongly opposed by the woman, and requested to delay that operation.

The infant was afterwards taken away by the nurse: thus nothing better was devised than emollient poultices for the boil, and a few depleting and absorbent doses. I had strictly enjoined, likewise, that the expected breaking and discharge of matter should be well attended, and related to me for further prescription. Indeed, three weeks after, good account was given of the suppurated boil; but a new alarm having been conceived from a return of the eruption, I was called for a

second examination, when, to my great regret, I found a hole on the margin of the anus, where I had seen the boil before. My probe was run into it at the depth of an inch and one-eighth. It was, in short, a complete fistula, on the right side of the rectum. The infant was now eight months and two weeks old. I tried with an indispensable violence to introduce my little finger in the anus, to feel and follow the probe as deep as it went, and by a small seringe I ascertained that the sinus was opened in the bowel.

This case was simple, nor could I apprehend much difficulty for an operation. I judged, however, that the space on which the fistula extended was so great, comparatively to the size of the subject, that the division of some arterial branch could not be avoided, and would leave a troublesome hæmorrhage. Besides I had not, nor did I know any precedent of a similar case whatever, to remove the idea of a possible danger, on, or after the operation. I therefore thought proper to request the advice and concurrence of our worthy and skilled Dr. Valentine Seaman.

He, by all means, encouraged the operation; but he wished that, a week or ten days before, a small dose of calomel should be daily given to the infant, with the view of counteracting or destroying any specific cause of eruption, if any could exist; indeed there was none to which he had been visibly exposed, yet the appearances were exciting some suspicion.

This mode was adopted with real advantage, for it rapidly cleared off the eruption. With this preparation, care was taken to keep the bowels clean, and to prevent accumulation of matter in the fistula, by daily injections. A gentle compression was also produced over it by a small tent, to be re-

newed from time to time. These means checked the progress of the fistula, and removed every symptom of inflammation; when, in presence of Dr. Seaman, I then performed the operation in the following manner.

Having laid the infant on his belly, in the lap of a woman, the nates being gently separated by two hands, I introduced in the anus a steel-grooved canula, directing the curve end of it towards the fistula. Holding it steady with the left hand, I introduced, with the other, in the sinus, a narrow sharp-pointed knife, the end of which was made blunt with wax: this was likewise curved in the same proportion as the canula. Now, attending carefully to the equal portion of both instruments left out, and to their parallel direction, I soon felt that their ends had met together. By a gentle pressure, the sharp-pointed knife easily made its way through the wax and the rectum, to lodge itself in the groove of the canula. In the closed end of this, having fixed and well adjusted the knife, I quickly and steadily brought out both instruments, without altering in the least their respective situation. Thus the rectum and the sphincter must have been perfectly divided, on the whole length of the fistula, which was immediately proved, by introducing a probe to the bottom of it, which could be lodged in the rectum, by removing it on the opposite side, without any obstacle. Much blood had gushed out at first, which was easily and almost naturally stopped, before a tent of a proportionate size was introduced.

In less than five weeks the cure was accomplished, by the method of frequent injections, and that of a small pledget softened with cerate and kept in the anus. Nothing remained now that could indicate the least internal soreness or ulceration;

and the visible indenture of the sphincter is perfectly healed and hardened.

REMARKS.

1. The precaution of counteracting whatever specific cause of eruption could be suspected, was a very wise one, notwithstanding we could not find out the least induction to surmise it. Calomel has done good, because it is a stimulant, happily counteracting that kind of atony or debility to which infants are very subject under the influence of various slight causes.

2. This case is a warning against the neglect of a boil formed on the margin of the anus. Had this been well opened and discharged at first, no fistula would have taken place. One half of these kinds of internal ulcerations, I am satisfied, originate with boils on that part, the confined and compressed state of which causes the diffusion of matter internally and through the tender cellular texture into which the rectum is affixed.

3. A fistula, in infants, must make very rapid and alarming progress. This case was accomplished during the short space of three weeks.

An infant was offered to my inspection, in Philadelphia, during the fall of 1793, who, from an inch behind the anus, on the right side, to one inch on the perinæum, had the whole opened, corroded, and destroyed, forming a large cavity, and horrid ulcer. I have a right to suppose that it originated with a fistula.

4. It is, perhaps, deserving some attention and inquiry, why this sort of disease does not occur on the left side as often as

on the right. The fistula of this infant was on the right side, and, among the considerable number of cases I have operated upon, and many more that I have inspected, I remember of none that was on the left side, although I have seen complicated fistulas with sinuses running upwards towards the coccyx, or downwards in perinæo, towards the scrotum. In the case of Mr. William M'Kenny, jun. of this city, the fistula originated with a chicken bone, transversely lodged above the sphincter. This was found late after he had applied to medical aid. From a very large cavity on the side of the rectum, five sinuses were running outwardly; but one of them had been so much enlarged, that it had become a false anus, occasionally discharging fæces and wind. All this train of shocking ulcerations, which was perfectly cured, existed on the right side, with the exception of one sinus running downwards under the line of the *raphe*.

5. The operation in this case was so easily performed by the means of a curved canula, that I would recommend this mode in preference to introducing the finger, which might put the parts upon such a stretch as to cause distention and much irritation. I observed likewise, that simple dressings and injections, made with the greatest care, were apt to produce some excoriations. Whenever it so happened, I omitted them one or two days, and applied only an alcoholic lotion.

New York, June 18, 1809.

Extract of a Letter from Dr. Pascalis *to the* Editor, *on the same Subject, dated New York, September* 3, 1809.

When I received the favour of your last letter, I was just beginning the treatment of a new case of fistula in ano, the deepest I have seen, which was seated exactly on the left side. I operated on it a few days after; but I retained some regret, lest my observations in the last piece you have been pleased to insert in the Museum, were too exclusive of the occurrences of fistula on any side of the sphincter. It remains, nevertheless, a strong opinion with me, that fistulæ in ano are more frequent on the right than on the left side, owing to the more common and usual way of lying on the right side during rest and digestion, which is thereby assisted by the weight of the aliments on the pylorus itself. If this last observation is correct, there has been something more than *accidental* in the constant occurrence with me of fistulæ on the right side.

Felix Pascalis.

Dr. J. R. Coxe.

Observations on the Formation of Æther, and a Proposition for obtaining it at one Operation; with Sundry Reflections. By James Cutbush.

ACCORDING to the generally received opinion of the formation of æther, it appears, that the alcohol undergoes a decomposition, by the action of the acid employed, and that several new products are formed.

From the experiments of Mr. Cruikshank we learn, that the proportion of carbon to hydrogen in alcohol is as 8 or 9 to

1, and in æther as 5 to 1. Therefore an abstraction of a part of the carbon in alcohol must take place before æther is generated; and the manner by which that abstraction ensues is obvious, from the necessary employment of an acid.

Is it not then from this circumstance, that æther, obtained by the distillation of alcohol and nitric acid, must contain less carbon than that formed by sulphuric acid and alcohol? Is the superior volatility of nitric æther a conformation of this principle?

During the distillation of alcohol and sulphuric acid, we obtain a product of æther, alcohol, acetous acid, water, oil of wine, and a peculiar species of carburetted hydrogen gas (usually called *olefiant gas*), accompanied with sulphurous acid; of which more particular notice will be taken. The sulphurous acid, particularly, is subsequently separated by distillation from caustic potash, sulphite of potash being formed.

The second operation has received the name of rectification, as the æther is obtained nearly pure; on washing it, however, the separation of alcohol (I presume), and any portion of sulphurous acid, which passed the second process, is effected; and the æther becomes of so great a strength as to effect the solution of elastic resin. Instead of rectification by means of potash, would it not be advisable to distil the mixture of alcohol and acid from a small quantity of oxid of manganese? Would we not obtain the product at one operation, uncontaminated with sulphuric acid, were this mode to be adopted?

From the circumstance of sulphurous acid being produced, it is certainly a fact that the acid is decomposed; that this decomposition is essential to the *formation* of æther appears to

me a subject of further research. The sulphurous acid, thus produced, passes over into the recipient, and is there condensed.

The object, therefore, of the subsequent distillation from *pure kali*, as prescribed by the dispensatory, is, as we have remarked, to separate the sulphurous acid, and probably some other products; but it is absolutely necessary that the separation of this acid should be accomplished, which can only be effected by this or a similar treatment, before it is proper for exhibition as a medicine.

In the employment of oxid of manganese, or the oxid of lead, no sulphurous acid would be formed; and it appears, that were the materials to be pure and in proper proportion, there would be no occasion for a second distillation; for the quantity of oxygen given out from the oxid, would be sufficient for the conversion of the sulphurous into the sulphuric acid; but, at the same time, the sulphuric acid would not be prevented from performing its *office*. Sulphurous acid, it is well known, will assume the gaseous state; but the heat necessary for the evaporation of sulphuric acid must be at least 500° of Fahrenheit: consequently it will not distil in the temperature necessary for the distillation of æther.

The formula which has lately been adopted for the distillation of nitric acid, furnishes us with an example of the utility of oxid of manganese in the distillation of some substances, which require more oxygen; and the facility with which this substance parts with its oxygen highly recommends it for the purpose. The nitric acid, we know, is a combination of azote and oxygen, and the nitrous acid is composed of the same elements, holding nitric oxid in solution: hence, as the latter

only requires an addition of oxygen to form the nitric acid, if we distil a mixture of nitrate of potash, oxid of manganese, and sulphuric acid, we obtain the nitric acid of a perfect state of purity, which, however, is influenced by the purity of the materials*. The oxid of manganese, in this instance, acts in the same manner as it would in the distillation of sulphuric æther.

The oxygen, which the oxid gives up on the application of heat, acidifies the nitric oxid, and forms the nitric acid: consequently no nitrous acid is produced.

I am aware that there are some instances in the preparation of æther, where the employment of oxid of manganese would be improper; and then it becomes necessary to use potash and a subsequent distillation.

In preparing nitric æther, by following the process described by Dr. Black, namely, by mixing the acid and alcohol gradually, subsequent distillation from *caustic potash* is certainly advisable, to separate the redundant acid. But the doctor prepares it merely by mixture.

If oxid of manganese were employed in the process of distilling a mixture of alcohol and nitric acid, in the preparation of nitric æther, it would act in this instance by changing any portion of nitric oxid, which was formed, into nitric acid. But in the preparation of muriatic æther, which is prepared

* For an account of obtaining nitre from various sources, and the mode of working the saltpetre caves in the western country, with a variety of remarks on the formation of gun-powder, &c. I beg leave to refer the reader to a series of numbers published in the Aurora, by me, entitled, "Application of Chemistry to Arts and Manufactures."

more economically by the distillation of a mixture of muriate of soda, sulphuric acid, and alcohol, it would change the muriatic into the oxymuriatic acid.

It may not be improper to conclude, by making some observations on the subject in general.

The decomposition of alcohol by the action of acids, and the production of new compounds, furnish matter of the utmost importance in chemical inquiry; and, indeed, no chemical philosophers have so ably treated the subject as Fourcroy and Vauquelin.

To these able chemists the scientific world is indebted for the present theory of the formation of æther. They state, that the *acid remains unchanged;* but that the alcohol is converted into æther, water, and charcoal. They remark, that when the production of æther *ceases*, the sulphuric acid is decomposed, and sulphurous vapour begins to arise, which condenses in irregular streaks, or in drops.

In consequence of this circumstance, the process should be immediately stopped, or the receiver changed. We have said, that, in the distillation of sulphuric æther, we obtain sulphurous acid, acetous acid, water, and oil of wine, which is accompanied by a peculiar species of carburetted hydrogen gas; and which, on account of its property of producing an oil when mixed with oxygenized muriatic acid, the German chemists have named olefiant gas.

It is said, that, if the process is stopped before sulphurous acid is formed (adopting the theory of the French), the *whole* acid, diluted with water and mixed with charcoal, remains in

the retort. Proust has also observed, that the sulphuric acid may be obtained from the black residuum, by diluting it with water, filtering it through linen, and evaporating it till it acquires the specific gravity 1.84; then adding about a five hundredth part of nitrate of potash, and continuing the evaporation until the acid becomes perfectly colourless, and acquires the specific gravity 1.86.

If, then, the above theory be adopted, which we have no *very material* reason to doubt, what is the *modus operandi* of the process?

Does the heat possess an affinity for a compound of carbon and hydrogen, containing less carbon than alcohol, which portion the acid retains? or does the acid act in the manner previously stated? The first principle *may* be observed, and it may happen, that, after the distillation of the æther, the carbon may decompose the acid. Then we would obtain another product, which is carbonic acid; for, in consequence of a portion of oxygen being abstracted, a part of the carbon would afford carbonic acid, and the sulphuric the sulphurous acid. Another compound of carbon and hydrogen must unite with another portion of oxygen, and produce acetous acid; and the olefiant gas must arise from a solution of carbon in hydrogen, which, in all probability, has originated from the alcohol, subsequent to the production of æther. The carburetted hydrogen, by uniting with caloric, formed the olefiant gas*. The water formed may also originate from the union of oxygen from the sulphuric acid, and hydrogen from the alcohol; so, also, the union of hydrogen, carbon, and some æther, may produce the ætherial oil of wine: and thus the several pro-

* For an account of the olefiant gas, see Ann. de Chim. tom. xxi.

ducts may arise from the reciprocal action of sulphuric acid and alcohol.

Since reflecting on the above, an idea struck me, which is, whether or not the sulphuric acid acts *also* by retaining a quantity of water, which the spirit of wine usually employed contains.

This, however, I find to be the opinion of Fourcroy and Vauquelin, according to Parkinson, which work, subsequent to writing the above paragraph, I had reference to. This gentleman states, page 143 of his Memoranda Chemica, that "Fourcroy and Vauquelin attribute the formation of æther to the *attraction* of the sulphuric acid for the *water* of the alcohol." But the experiments of Mr. Cruikshank, on the proportion of carbon in alcohol and æther, make it appear, that there is a difference in the quantity of the carbonaceous principle in æther and alcohol; consequently an abstraction of carbon must take place, independent of the observation of Fourcroy and Vauquelin, before this fluid can be formed.

According to a former theory, it is stated by some chemical writers, that æther was nothing more than oxygenized alcohol; and they remarked, that it was formed of part of the oxygen of the acid united with the carbon and hydrogen of the alcohol.

Gren observes, speaking of the distillation of a mixture of *muriate of soda*, *magnesian oxid*, *alcohol*, and *sulphuric acid*, that a dulcified oxymuriatic acid is obtained, and an oily product which he called *oil of salt;* but if oxymuriate of potash and alcohol are distilled, a muriatic æther, according to Van Mons, is obtained.

It has always remained a difficult matter to prepare nitric æther; to devote a portion of our time to this subject, at a future period, will not be improper. It was my intention to have given observations on the preparation of the æthers respectively; but, in consequence of the length of this essay, I purpose performing this task on a future occasion.

An Essay on Insensible Perspiration, and the supposed Absorption and Emission of Gases by the Surface of the Human Body. By* JOSEPH KLAPP, *M. D.*

INSENSIBLE perspiration, on account of its influence on the healthy and diseased conditions of the system, is a subject of importance to the practitioner of medicine; and, as it is one of the number of intricate functions belonging to the animal body, it is eminently entitled to the attention of the physiologist.

At first sight, this inquiry may to some seem unnecessary, as it has already been ingeniously and elaborately pursued by many writers, particularly so by Sanctorius, Keil, and Cruikshank. But when it is recollected that this office of the skin is more considerably influenced by climate than by any other incidental circumstance to whose action it is exposed, it cannot seriously be supposed, that the subject has yet been exhausted. The observations and experiments of those authors may have disclosed every thing necessary to be known concerning perspiration in their respective countries, but I question whether their account of this function will be found to apply with equal propriety and accuracy in the climate of America. I am de-

* A part of this essay was read before the Medical Lyceum, at one of the meetings of last winter.

cidedly of the opinion, that, until the example which is set for us in the indefatigable labours of the great Sanctorius be followed in every climate of the world where our science has been extended, the physiologist must remain in a considerable degree ignorant of a general knowledge of perspiration.

A few authors, ancient and modern, have contended that the skin performs an analogous or subsidiary office to the pulmonary organs. They suppose, that, like the lungs, the surface of the human body *transpires* and *inhales* different kinds of gases. Had this reputed or imaginary discovery been confirmed by the experiments of other persons, it certainly would have constituted a new and highly interesting æra in the physiological science. But that some delusive circumstances or other have led these writers into a great and serious error, I conceive there can exist no doubt. My experience, which will be detailed presently, united with that of a few other writers, authorize me to assert with confidence, that gases are neither transpired nor inhaled through the surface of the human body.

The points I am particularly desirous of investigating, may be resolved into four general heads:

I. What quantity of insensible perspiration is emitted by the skin in a given space of time?

II. What are the sensible and chemical qualities of the matter of perspiration?

III. What affinity exists between the aqueous substance transpired by the skin, and that thrown out by the lungs in respiration?

IV. Are gases transpired and inhaled through the surface of the human body?

The *skin*, or investment of the human body, has usually been divided into three membranes, the cuticle, rete mucosum, and cutis vera. The conformation of the most exterior of these membranes, the scarf-skin as it is called, has been differently described by different authors. That it is composed of the lays of scales, analogous to the arrangement of the scales of fishes, is the theory of Lewenhoek. According to professor Mickel, it is a mucous substance, thrown out by the extremities of the cutaneous vessels, and hardened by the air of the atmosphere. Morgagni says, it consists of a callosity of the extreme vessels of the skin; but some later writers have discarded all these theories, and contend that the cuticle is neither lays of scales, exsiccated mucus, nor a callosity of the vessels, but is an organized membrane formed by the animal economy, in a like manner with other vital parts of the body.

Lewenhoek supposed the matter of perspiration was deposited by exhalent arteries under the cuticle, and was from thence discharged through the interstices or loose edges of the scales of the cuticle; and Mickel has asserted, that the scarf-skin is an entire membrane, and the perspirable fluid passes through it by a process of soaking, or transudation. Mr. Cruikshank, however, embraced a very different theory. He supposed the cuticle was not an entire, but a porous membrane, and that the discharges of the skin pass through certain imperceivable vessels, which run from the perspiratory vessels of the cutis, and terminate on the surface of the epidermis. It may not be improper to observe, that Mr. Cruikshank founded this opinion on what seemed to him plausible, and not on such obvious and certain grounds as could warrant its expression

with positiveness or great confidence. He relates facts, and uses much reasoning, to prove the *vitality* of the cuticle, but very candidly acknowledges, that, "after some pains, and assisted by pretty good microscopes, he was not able to discover perforations in the cuticle, or rete mucosum."

The *rete mucosum* appears, as might be supposed from its name, to be a mucous substance, bearing a resemblance to network, in consequence of certain impressions on its surface. It is situated between the epidermis, just described, and the true skin. It is the principal, if not the exclusive seat, of that great variety of complexion, which distinguishes the human inhabitants of different quarters of the world. The same dispute concerning the nature and formation of the cuticle, exists with regard to the rete mucosum. The common opinion of former anatomists is, that this membrane is not vascular; and, perhaps, inasmuch as relates to *plain* and *obvious* data, we have full as much reason to credit this ancient theory of Albinus, as we have that of Cruikshank, and some other modern authors. Mr. Baynham believed he had succeeded in injecting rete mucosum; but Mr. Cruikshank informs us, that Mr. Baynham was good enough to leave with him for some time his preparation, and that, after mature deliberation, and close inspection, he convinced himself that it was not this membrane which Mr. Baynham had injected. Malpigi, the discoverer of this "corpus mucosum et reticulare," as he calls it, asserted that he had discovered *tubes* or *vessels* in it; but succeeding anatomists, who were possessed of equal opportunities of information with himself, have attentively sought for these perforations without success. Mr. Cruikshank was of the opinion, though Mr. Baynham did not discover vascularity in the rete mucosum, his injection had given rise to the discovery of a membrane hitherto unknown to anatomists. From the time

the knowledge of Mr. Baynham's injection reached Mr. Cruikshank, it seems as if he had been spurred on to a very assiduous investigation of the subject. He says he was repeatedly successful with the skin of persons who had died of natural small-pox, in passing injecting matter into a membrane which occupies the space between the rete mucosum and the cutis. This membrane he calls the cuticula quarta, which he supposes is the particular seat of small-pox, and other eruptive diseases. He supposes this is the membrane which Mr. Baynham injected, and acknowledges, with some flattering compliments to his talents and rising fame, that he is the first discoverer of it.

Relative to the *cutis vera*, there can be no doubt as to its vascularity. It may be described to be a white, exceedingly vascular and sensible membrane. It is the organ which performs the important offices of touch or feeling, and of perspiration. Its characteristic or distinguishing properties are, its uniform whiteness, its vascularity, sensibility, and it is said to discover, under some circumstances, marks of elasticity. Different classes of porous openings on the cutis are mentioned by anatomists ; the ducts of the sebaceous glands, the ducts which convey the hairs, and the perspiratory vessels.

It has been supposed, as I have before intimated, that the matter of perspiration is conveyed through certain processes or vessels, which run through the cuticle and rete mucosum, and are attached to, or inserted in, the openings of the exhalent arteries on the surface of the true skin. But this opinion is a theory with Mr. Cruikshank which can claim no other support or authority, than what it has derived from his ingenuity. It may possibly be, that the rete mucosum and cuticle are vital membranes, and possessed of both exhaling and absorbing

vessels, but anatomists as yet have fallen far short in their exertions of proving either the one or the other. At present, I sincerely believe the fact is, though many theories have been proposed to explain the manner the perspirable fluid is taken up from the extremities of the exhalent vessels in the cutis, and conveyed to the external cuticular surface, still the question is an undecided point. It is impossible, in the present state of our knowledge, to conclude with any degree of certainty which of the above opinions are true, or that any one of them is so. The true explanation is perhaps reserved for some future period, when the anatomical study may be pursued under auspices more conducive to its progress.

Having made these preliminary remarks, I shall next undertake the prosecution of the first object of the present inquiry:

I. What quantity of insensible perspiration is emitted by the skin in a given space of time?

Sanctorius and Keil calculated the quantity in four and twenty hours, by weighing the body before and after sensible evacuations in the morning. Though I have never possessed the convenience of the balance used by those experimentalists, yet I must confess, making a due allowance for the halitus of the lungs, it seems to be calculated to afford a more accurate estimation of the degree of the discharge, than any other method with which I am acquainted; not excepting even the one I have at present adopted. It was with regret I found it necessary to relinquish a previous intention of making experiments on a large scale, and to pursue what I must term the less certain method of Mr. Cruikshank. Weighing the actual vapour of perspiration, collected from a small portion of the whole surface of the body, may have seemed to that gentleman

as the best way of investigating the subject, but though I have no doubt it will serve to throw much light on the doctrine of perspiration, yet I never can recommend it so highly as he has done. I cannot avoid being sensible in my own mind, that *the degree of perspiration of the same parts, varies at different times; and also that the quantity of perspirable fluid emitted by the different parts of the whole cuticular surface at the same time, is considerably different.*

EXPERIMENT I.

Fahrenheit's Thermometer 70°.

Two hours *after dining*, my hand was introduced into a wide-mouthed bottle, which would contain about half a gallon. To intercept all communication between the air without and that within the bottle, a piece of varnished silk was closely fastened around the mouth of the bottle, and, on introducing my hand, the other end of the silk was, with a piece of wide tape, fastened to my wrist. To answer this purpose, Mr. Cruikshank used part of a bladder. But having ascertained that a moistened bladder, in a warm temperature of the air, will, in a short time, form fixed air, which might give rise to a deception, I was of the opinion it would be improper to use it. In this way, perhaps, the fixed air, which Mr. Cruikshank believed he found in the bottle after withdrawing his foot, was produced; at any rate I shall presently relate many experiments, which show that in all probability it could not have been discharged by the skin, as he imagined. The statement, or rather the opinion of Mr. Cruikshank on this point should be cautiously received, as his experiments were afterwards frequently repeated, by that veteran in philosophy, Dr. Priestley, without on any occasion discovering the least fixed air. I should be very sorry to be understood to impeach the veracity

of so respectable an author as Mr. Cruikshank was; and, in order that so unjust an impression may not be taken, I now declare, that my only meaning or wish is to suggest that he may have been mistaken, in imputing the transpiration of fixed air to the skin, and in fact he may have been deceived with regard to the fixed air altogether.

In the course of a few minutes after placing my hand in the bottle, its transparency was effaced by the abundance of the vapours of perspiration; and in a few minutes more the internal surface of the bottle was covered with small drops of these vapours, condensed into a liquid. To promote the running together of these drops, and their descent into the bottom of the bottle, I had recourse to the means used by Mr. Cruikshank. A linen cloth, dipped in cold water, was wrapped equably around the bottle, with the view of condensing the vapours within, by producing a process of gentle evaporation from the external surface of the bottle. The application of the wet cloth accomplished the desired effect; for the vapours of perspiration were pretty soon converted into a liquid.

At the expiration of an hour, my hand was withdrawn, and, to all appearance, there was collected a large tea-spoonful of transparent liquid. My first desire was to ascertain whether fixed air had been thrown out by my hand into the air contained by the bottle. As I have mentioned, Mr. Cruikshank supposed such was the case in his experiments, or that something, which he believed might be phlogiston, was discharged by the skin, and, by uniting with a portion of atmospheric air in the bottle, formed fixed air. A lighted taper was directly on withdrawing my hand introduced into the bottle; but the flame was not thereby in the least diminished. About a half ounce measure of the air was next passed through lime water, and, though it

was agitated in the water, the volume of the air was not diminished, nor were white clouds produced in the lime water. I think I may therefore say, carbonic acid gas was not thrown out by my hand.

On weighing the liquid collected in the bottom of the bottle, I found it was equal in weight to twenty-seven grains, according to a very accurate pair of scales. Therefore, proceeding to calculate agreeably to Mr. Cruikshank's rule, it is proper to say, if the surface of my hand is to the surface of the whole body as one is to sixty, and if the perspiration of all parts be equal, then in twenty-four hours six pounds and nine ounces of insensible perspiration were emitted by the skin.

EXPERIMENT II.

Fahrenheit's Thermometer 60°.

In the morning, one hour *before breakfast*, my hand was introduced into the bottle, and confined with the varnished silk as before described. The matter of perspiration collected on the surface of the bottle in a very sparing and tardy manner. When one hour had expired, my hand was withdrawn; and although the evaporation of water from the external surface of the bottle was used some time previous to the discontinuing of the experiment, yet the quantity of the condensed perspiration collected did not exceed a few drops. To ascertain whether any fixed air had been emitted by the skin, a lighted taper was introduced into the bottle, but without occasioning any alteration or diminution of its flame. Some of the air of the bottle was next examined over lime water, but no discoloration of this chemical test was perceived to occur. The few drops of condensed insensible perspiration at the bottom of the bottle were poured into the scale of a balance which contained a bit of

sponge, the exact weight of which had been previously ascertained. With this piece of sponge the small quantity of perspiration still adhering to the inside of the bottle was imbibed with much care, when it was again returned to the scale. After deducting the weight of the sponge, the quantity of insensible perspiration collected was only six grains. It may therefore be said, if the surface of the hand is to the whole surface of the body as one is to sixty, and if the perspiration of all parts be equal, then, during four and twenty hours, in a moderate temperature of the atmosphere, while the excitement of the system is languid, and the stomach empty, the quantity of insensible perspiration in twenty-four hours is only eighteen ounces.

This experiment, compared with the others, will serve to show, that the discharge by the cutaneous passages is different at different hours of the day, less fasting than after eating; or, in other words, it will show that the discharge varies in degree as certain causes more or less concur to influence it.

EXPERIMENT III.

Fahrenheit's Thermometer 80°.

Two hours *after breakfasting*, my hand was again introduced into the bottle, and embraced with the varnished silk as in the other experiments.

The vapours of insensible perspiration, as usual, in a short time began to collect on the surface of the bottle, and, on being resolved into a liquid state, gradually passed down into its bottom. The experiment was continued one hour, and in ten or twelve minutes previous to its conclusion, the wet cloth was applied equably around the bottle. On withdrawing my hand,

I proceeded to weigh the condensed perspirable fluid, not deeming it necessary to bestow any additional attention to the chemical quality of the air in the bottle. The quantity collected proved to be fourteen grains; therefore, with the provisions before stated concerning the cuticular surface, and the perspiration of different parts, it is proper to say, in twenty-four hours forty-two ounces of insensible perspiration were thrown out by the cutaneous passages.

EXPERIMENT IV.

Fahrenheit's Thermometer 75°.

One hour *previous to dining*, I executed the present experiment, with the same apparatus before used. My intention in selecting different times of the day for the making of the present experiments, is to endeavour to discover what influence the state of the stomach, as to emptiness, and its containing aliment, has on the extent of the discharge by the skin. When one hour had expired, I withdrew my hand from the bottle, and poured out the condensed perspirable fluid into the scale. Its weight was twelve grains. If, then, the perspiration of all parts be equal, and the surface of my hand is to the surface of my whole body as one is to sixty, in twenty-four hours I lost thirty-six ounces of insensible perspiration by the skin.

EXPERIMENT V.

Fahrenheit's Thermometer 90°.

Mr. R. C., at my request, *immediately after dining*, introduced his hand into the bottle, and intercepted the communication of the external air in the usual manner. The wet linen cloth was applied to condense the vapours. The hand was removed in one hour; and, on pouring the perspirable liquid

into the scale, it appeared twenty-two grains had been collected, which, multiplied by sixty, and then by twenty-four, for the whole cuticular surface, and the given time, would make the extent of this discharge, allowing it to be the same at all times, and equal from every part, sixty-six ounces.

By Sanctorius, the average quantity of insensible perspiration discharged by the skin *in Italy*, was estimated at fifty ounces in twenty-four hours; by Keil, of Northampton, *England*, it was calculated to be thirty-one ounces; and Mr. Cruikshank, of the same country, has made it nearly three times the latter quantity. If the method of experimenting which I have adopted be just, and if I have made no mistakes in its execution, it will be easy to perceive the degree of insensible perspiration by the skin in the climate of *the United States*. From much attentive observation, and many experiments on the subject, made in different states of the atmosphere, and in different months of the year, and comprizing both what is contained in this essay, and what I have not conceived necessary to mention, I am induced to believe, that the average quantity of insensible perspiration, in this country, is somewhere about forty-one or forty-two ounces in twenty-four hours. It must not, however, be forgotten, that the proverb, "circumstances alter cases," which is very commonly mentioned in speaking of small matters, is perfectly applicable to this office of the skin. Age, constitution, temperature of the air, diet, exercise, and a multiplicity of other causes, have no inconsiderable influence over the insensible perspiration.

II. What are the sensible and chemical qualities of the matter of perspiration?

The following appear to be its principal sensible qualities: it is limpid, tasteless, and in liquidity it is about the same with water. It is somewhat lighter than water, as was ascertained by the next experiment.

EXPERIMENT VI.

Of condensed insensible perspiration, ten drops were placed in one scale of a very delicate balance, and an equal number of drops of water were placed in the other scale. The drops of both liquids were as nearly of the same size as could well be obtained. They were dropped out of the same vial, and by the same hand. On raising the beam, the scale which contained the water preponderated. The difference between the weight of the two fluids proved to be three grains.

My convenience has not been such as would enable me to say much concerning the chemical properties of the insensible perspiration. I have however experimented so far, as to ascertain that it is possessed of neither *acid*, *alkaline*, nor *astringent qualities*.

EXPERIMENT VII.

Some of the tincture of blue cabbage was poured into a vessel, which contained a small, but sufficient quantity of condensed perspiration, without producing either a red or a green colour. I therefore concluded it did not contain either an acid or an alkaline property.

EXPERIMENT VIII.

Some of the solution of oxy-sulphat of iron was added to a small quantity of condensed insensible perspiration, but without occasioning a black colour, or the least precipitation of the metal. From which circumstance it was inferred, that the matter of insensible perspiration is not possessed of an astringent quality.

III. What affinity exists between the aqueous substance transpired by the skin, and that thrown out by the lungs in respiration?

To investigate this subject, it was of course necessary to collect the aqueous matter thrown out by the lungs in expiration; and as the *quantity* of this discharge could be at the same time so easily ascertained, I resolved to include this additional object in my experiments. The method of these experiments was simply this: one end of the silk investment before used was fastened to the neck of a half gallon bottle, and in the other end was included a tin tube, about a foot in length. An apparatus of this description would enable me to empty my lungs, through the tube, into the bottle, in order to condense and to collect the halitus, and at the same time afforded me the advantage of not being incommoded by the non-respirable gases, thrown out by the pulmonary organs.

EXPERIMENT IX.

At three o'clock, P. M. I commenced collecting the aqueous discharge of the lungs, by expiring through the tube into the bottle, and inspiring the air from the atmosphere, which was

necessary for the important purposes of respiration. In a short time the condensed halitus began to run down the internal surface of the bottle, and to collect in its bottom. At the expiration of half an hour the process was discontinued; when, according to my scales, I had collected one drachm of liquid from the air thrown out by the lungs.

The whole of this discharge, then, for twenty-four hours, would be six ounces, allowing it to be the same in degree at all times. I next proceeded to ascertain what analogy existed between the sensible and chemical qualities of this discharge, and that of the skin. In appearance, liquidity, and taste, I was not able to discover any difference between them. Like the vapours of insensible perspiration, the halitus of the lungs, by condensation, forms a *thin*, *tasteless*, and *limpid liquid.* Lime water added to it occasioned no precipitation; nor does it change the colour of the tincture of blue cabbage. No gallat of iron was formed, on adding to it some of the solution of the oxy-sulphat of iron. This experiment was afterwards frequently repeated, with the same result, excepting what relates to the quantity of the aqueous matter emitted, and even in this respect the variation was so trifling, as scarcely to deserve mentioning. One of these variations, perhaps, it may be proper to relate.

EXPERIMENT X.

At half past 12 o'clock, P. M. I began this experiment; the same apparatus was used as described in the last. The halitus from the lungs, as usual, in a few minutes presented itself in a liquid state on the internal surface of the bottle. The process was continued precisely half an hour. The condensed liquid was poured into the scale of the balance; and its weight was ascertained to be sixty-nine grains. The addition of lime

water, tincture of blue cabbage, and oxy-sulphat of iron, to the condensed liquid, was followed by the same results as mentioned in the ninth experiment. The liquid was *limpid*, *tasteless*, and possessed of neither *acid*, *alkaline*, nor *astringent* properties.

From these experiments I have concluded, that there exists no difference between the liquid afforded by the halitus of the lungs, and that which is formed out of the vapours of insensible perspiration. Perhaps the process by which they are formed and discharged from the system, is the same. The two following reasons incline me to suspect that the halitus of respiration is formed by a process of secretion in the lungs, and not by a chymical combination, as is very commonly believed. 1st, The natural temperature of the lungs seems insufficient to excite the affinity of combination between the constituent gases of the halitus. 2d, The vapours of insensible perspiration, which afford by condensation the same substance with the halitus of the pulmonary organs, are certainly the product of a vital process of secretion in the skin; and as the substance or liquid is formed by glandular or organic action in the one instance, in all probability it is in the second formed in the same manner.

To be continued.

MEDICAL MUSEUM.

VOL. VI.....No. IV.

A Case of Variolous Fever, showing the Good Effects of Blood-letting. In a Letter from Dr. THOMAS J. WINDER, *Carthage, Somerset County, Maryland, to Dr.* JOHN R. COXE.

I FORWARD to you, for your Museum, a few facts relative to a case of small-pox; and should they prove the means of arresting the arm of death in similar cases, the intention of the author will be amply repaid. And although they are not of a very recent date, yet they may be no less useful on that account.

In the spring of 1796, I was requested by the venerable Dr. Leven Irving to visit, in company with himself, a Mrs. Harrison, of an emaciated habit, about 55 years of age, of Salisbury, whom he informed me was labouring under a violent fever. When we saw her, we found the fever high, and of an inflammatory kind. It immediately struck me that it was the variolous fever, and I communicated that opinion to the doctor; for it was but a few days before that I discharged a house of inoculation about one mile from town. It was mutually agreed on, that she should be removed instantly out of town to that house, when the next day too well proved that our suspicions were well grounded. An eruption came on, with violent

symptoms of inflamed fauces, and difficult deglutition, &c. The eruption was very thick, and, as the pustules progressed, they sunk and put on that black appearance which Dr. Cullen and his predecessors say evidently indicates a great putrescency and dissolution of the fluids, and for which they recommend the free use of bark, wine, the elix. vit. &c. When the patient was removed, the care of her devolved solely on myself; and, as I was averse to the practice of Dr. Cullen, having seen it tried fully, under favourable circumstances, without any good effect, a few years before that, in the state of Virginia, I therefore adopted the lancet, believing that those symptoms which Dr. Cullen and others believed to depend on a putrefaction of the fluids, indicate the very reverse, and evince, unquestionably, the presence of great inflammatory action. I was led to that opinion by Dr. B. Rush; and, however I differ from that illustrious professor on some points, I am ready to acknowledge my obligations to him for this hint, and many other principles in medicine that I have since made a part of the groundwork of my profession. But how often I bled in this case I cannot charge my memory, but I believe as often as is usual in a common pleurisy. Suffice it to say, that I founded the cure principally on the lancet and glysters, making use of gargles, diaphoretics, and medicines of less consideration, occasionally. The fact was, the pock rose and pustulated on the use of the lancet, and in a very short time I discharged my patient well, and the old woman was living not many weeks ago.

An Essay on Insensible Perspiration, and the supposed Absorption and Emission of Gases by the Surface of the Human Body. By JOSEPH KLAPP, *M. D.*

(Continued from page 224.)

IV. ARE gases transpired and inhaled through the surface of the human body?

Many of the experiments which I have to relate under this head of the investigation, are extracted from my Inaugural Essay, which was printed in this city, in the year 1805. My apology for introducing them in this place, is derived from the following considerations.

Some of them have been made, by a very respectable physiologist of London, with different results. On this account, and their important tendency in physiology, I wish to bring them more completely before the public, with the hope of being thereby able to stimulate some third person to ascertain, by repeating our experiments, who has committed the error. If I have been deceived, I certainly shall be thankful for a speedy and liberal correction, as truth is, to me, far more desirable than the retention of such opinions as I may have been induced to hastily and improperly adopt. In justice, however, to myself, it may be proper to state, that my experiments were not privately conducted, but in the presence of several persons conversant with the subject. But the gentlemen who particularly witnessed their performance, and to whom I conceive myself under some obligations for assistance in their execution, are the late Drs. Woodhouse and Hutchinson. From the former gentleman, I was honoured with a written

attestation of the accuracy and impartiality with which my experiments were conducted. I shall take the liberty of introducing this letter, as the reputable character which my lately deceased friend has left behind him, will, I am certain, protect me from all ungenerous suspicions.

May 3, 1805.

DEAR SIR,

It afforded me great satisfaction to witness the zeal and industry with which you conducted the inquiry which has led to the important deductions contained in your Inaugural Dissertation, disproving the existence of an aeriform function in the skin.

With pleasure, I bear witness to the neatness and accuracy with which your experiments were performed, and consider them as completely satisfactory on this interesting subject.

That the light which you have thrown on this question may stimulate you to extend your inquiries to other branches of the science of medicine, and that you may enjoy health and happiness in life, and meet with success in the exercise of your profession, is the ardent wish of,

Dear sir,

Your sincere and affectionate friend,

JAMES WOODHOUSE.

Mr. Joseph Klapp.

Having ascertained the nature of the gases contained in the pump-water of the Pennsylvania hospital, where some of my experiments where first performed, no doubt was entertained but that the inquiry would, in some degree, be promoted, by experimenting with my hand and arm held in a glass vessel

filled, and inverted in it. Accordingly, a few experiments were made in this manner; but, as water has been objected to, it may be as well to withhold them until unexceptionable ones are submitted. It may, however, in this place be mentioned, that the gases collected differed in no considerable degree from those which had been previously obtained from the water, by exposing it to a gradually increased temperature.

To promote the purposes of an inquiry like the present, it appears to me, no just or strongly marked exceptions can be taken to quicksilver, as a proper medium to experiment in. With this belief, the necessary quantity of this metallic fluid was procured, and a tub, with requisite conveniences, arranged for its reception. I am pretty confident, if any airs are thrown out by the skin, the fact can be easily ascertained, by immersing the hand and wrist in quicksilver; and, in case they should be transpired, their kind or nature can afterwards be detected by applying to them suitable chemical tests.

With these views I commenced the course of experiments now to be related.

EXPERIMENT XI.

Fahrenheit's Thermometer 73°.

After all loose atmospheric air which might be adhering to the surface of my hand and wrist was completely separated, by moving them in different directions under the surface of the quicksilver for ten or twelve minutes, they were introduced into a glass vessel which had been filled with and inverted in the quicksilver. In this state I continued for upwards of two hours. During the first few minutes of the immersion, my hand was in some degree benumbed with cold, but its usual

warmth soon returned; and, long before the above time was expired, I was sensible of little or no inconvenience, excepting what proceeded from the pressure of the mercury. Notwithstanding the length of time the immersion was continued, not a single bubble of either carbonic acid, azotic air, or of any other kind could be perceived to emanate from the skin.

This experiment was first instituted by me, about eight months ago, at the Pennsylvania hospital, in the presence of Dr. James Hutchinson, and was, at the present time, repeated in the chemical laboratory, in the presence of professor Woodhouse and others, with precisely the same result.

Lime water is considered a just, and I believe it is a pretty delicate test for carbonic acid; if the latter be excreted by the skin, it must, while the hand and wrist are immersed in the former, produce carbonat of lime, and the milky colour which thereby occurs, will serve as the indication of its transpiration.

EXPERIMENT XII.

Fahrenheit's Thermometer 70°.

My hand and wrist were introduced into a jar containing lime water, whose purity, or readiness to combine with fixed air, had been previously ascertained. The process was continued one hour. No carbonat of lime was formed; on the contrary, the lime water, on withdrawing my hand, was as transparent as it was before used. The passage of atmospheric air into the jar was guarded against, and, in every particular, the experiment was conducted with fairness and precision. Since it was first instituted, I have seen it repeated by different persons; and, in one instance, although the immersion was

continued double the above time, yet the result was the same as in the first trial.

This experiment, then, not only shows fixed air is not transpired by the skin, but it also proves the same thing with regard to other gases which are not possessed of an affinity to combine with lime water. Had any of them been discharged, they would have been seen rising in bubbles through the lime water, but this was not the case.

EXPERIMENT XIII.

Fahrenheit's Thermometer 56°.

Having half filled a convenient vessel with water, which had been boiled until it had parted with all the loose air it contained, my foot and ancle were then introduced into it, and retained in it for three hours. Now, as it is well known that boiled water absorbs carbonic acid with avidity, it will only be necessary, in order to discover whether that acid gas had been discharged by the skin, to find out, by a suitable test, whether the water had acquired any of it during the immersion of my foot and ankle in it; to ascertain which, a glass vessel was filled with, and inverted in it, over a large dish; a handful of fresh leaves, taken from a healthy plant, was then introduced into the inverted vessel, and the whole exposed to the rays of the sun for four hours. During that time, no oxygen air was disengaged from the water; therefore, it is very presumable, fixed air was not emitted by the skin. Every one, I suppose, is fully acquainted with what Dr. Woodhouse and others have established concerning the economy of vegetation, to wit: that the leaves of plants readily detect and decompose, by the aid of solar light, any carbonic acid contained by the water in which they are immersed. In this case, the operation of dou-

ble elective attraction takes place. The fixed air is acted on by the divellent forces of two causes. The plant attracts its carbon for nutrition, and the light combines with the oxygen to form oxygen air.

EXPERIMENT XIV.

Fahrenheit's Thermometer 56°.

Having procured some pure hydrogen air from diluted sulphuric acid and the filings of malleable iron, four ounce measures of it were thrown up into a glass vessel, previously filled and inverted in quicksilver. My hand and wrist, after all loose air was separated from their surface, as in a former experiment, were introduced into the inverted vessel, and retained in this situation for three hours. In this way, I experienced very little inconvenience from either pressure or coldness. In the presence of Dr. Woodhouse, the air in the vessel from which my hand had just been withdrawn, was examined. Having ascertained that the volume of the air had not been diminished, we passed up one ounce measure of it through lime water, in a eudiometer, but no milky appearance was by either of us observed to take place. About three ounces of lime water were then passed up into the vessel containing the remainder of the air; no carbonic acid, however, could be detected in either trial.

The foregoing experiments show, that gases are not *transpired* through the surface of the human body. I shall, in the next place, relate some experiments which have induced me to believe, that airs are not *absorbed* by it.

EXPERIMENT XV.

Fahrenheit's Thermometer 60°.

Five ounce measures of atmospheric air were thrown up into a glass vessel previously filled, and inverted in quicksilver. My hand and wrist, after the necessary precautions, were introduced into the vessel, and retained there with very little inconvenience from the mercury, during three hours. We immediately proceeded to analyze the air out of which my hand and wrist had just been taken: its volume had not been diminished; and, when passed through lime water, no milky colour ensued, which circumstance is an additional proof that the skin does not throw out carbonic acid gas. We next endeavoured to discover whether any of the two gases which compose atmospheric air had been taken up. For this purpose, one ounce measure of it was passed up over water, in a eudiometer charged with a bit of phosphorus; and, in less than twenty-four hours, the absorption of $\frac{22}{100}$ was complete.

This experiment alone clearly proves, that neither oxygen nor azotic air are absorbed through the surface; but, in order to afford every satisfaction on the subject I am able, the following varied ones are submitted.

EXPERIMENT XVI.

Fahrenheit's Thermometer 54°.

Five ounce measures of oxygen air of 4 per cent. purity having been transmitted into an inverted vessel filled with quicksilver, my hand and wrist, with the previous cautions, were introduced into it, and held in it for three hours. Upon examination, after withdrawing my hand, it appeared, that the

quantity of the air had experienced no diminution. It was then passed through lime water, and, as turbidness did not occur, we inferred that carbonic acid had not been given out by the skin. The air was next analyzed, as in the last experiment, with the eudiometer of Berthollet, and was found not in the least adulterated.

In order to ascertain whether or not fixed air be absorbed through the external surface, the following experiment was performed.

EXPERIMENT XVII.

Fahrenheit's Thermometer 61°.

Five ounce measures of carbonic acid, obtained from carbonat of lime, by diluted sulphuric acid, were transmitted into a glass vessel previously filled, and inverted in quicksilver. My hand and wrist were next introduced into it, and kept in this state for three hours. On withdrawing my hand, the volume of the air seemed somewhat diminished; perhaps to the extent of half a drachm; the removal of which, in all probability, was owing to its combination with the perspirable fluid. When the affinity is adverted to which subsists between this acid gas and all matters of an aqueous nature, the occurrence in the experiment which I have mentioned is, perhaps, what ought to have been expected.

The emission of airs by the skin was first contended for by certain authors, who had experimented in common water, without ever attending, or in the least adverting, to the water previously containing the very gases which they collected. The circumstance of different gases, such as oxygen, azote, carbonic acid, &c. being actually contained by common water, or the

most of it, and the disposition of those gases to rise out of the water when acted on by the heat constantly passing out of the skin, places such a construction on their experiments, as to greatly diminish the importance which has long been attached to them. The authors I here allude to are count De Milly, whose paper is recorded in the Memoirs of the Royal Academy of Sciences at Berlin, for the year 1777; Mr. Ingenhouz, whose brief and imperfect experiments may be found in his own works; and Mr. Trouset, a French writer, a translation of whose paper may be seen in one of the volumes of Nicholson's Journal. The two first authors have contended for the transpiration of carbonic acid gas, and the latter denies that this is the case, and adduces experiments to prove it is only azotic air which is emitted.

Dr. Rush used to relate, in his lectures, an experiment which he considered very favourable to the theory of the transpiration of gases by the skin. A lighted candle brought to him in the morning, before he had risen, was suddenly extinguished by introducing it into the air of the bed in which he had slept the preceding night: from which the professor concluded, that the air under the bed-clothes had been contaminated by the emission of either carbonic acid gas, or azotic air, by the skin. I have viewed this experiment in a different light. I can readily subscribe to the fact as related by the doctor, but cannot agree with him in his conclusion from it. The facility with which the process of respiration contaminates atmospheric air, by consuming its oxygen, is familiar to every one; and, in my opinion, the true solution of the experiment is, that the adulteration of the air in the bed was owing to this pulmonary process. This explanation will seem very probable, when it is recollected how liable we are to lie, now

and then, in the course of a night's sleep, with our heads under the bed-clothes.

The following facts, which rest on the foundation of experiments presently to be related, prove, that error is attachable not exclusively to De Milly and Ingenhouz, nor to Trousset, who controverts their experiments, but that all three of them are mistaken.

I. The gases collected by holding the hand and fore-arm in pump or common water, are of the same nature, and are nearly in the same proportions to each other, that they are in the water previous to its use.

II. If a part of the body be immersed in boiled or distilled water, which has been entirely deprived of all its loose air, gases of no description whatever are disengaged by the skin.

III. If the hand and fore-arm are invested with fine varnished silk, which is not penetrable or pervious to air, small bubbles of gases, corresponding with those previously contained by the water in which the experiment is made, will be seen to rise from the surface of the silk.

EXPERIMENT XVIII.

Fahrenheit's Thermometer 70°.

A Florence flask was better than half filled with the pump-water of the Pennsylvania hospital; and, after adapting a syphon to its mouth through a cork, it was placed by the side of a pneumatic tub, and a gradually augmented temperature applied. The extremity of the syphon communicated with a bell

glass filled with water. Globules of air, in a short time, began to rise through the water, and collect in the top of the bell glass. When a sufficient quantity of the air had been collected to admit an examination, with Dr. Hutchinson I proceeded to this process. It appeared to both of us, on subjecting it to the action of the common tests in the eudiometer, that the air consisted of carbonic acid, oxygen, and azote, nearly in the proportions to each other as in that which I had previously obtained by holding my hand and fore-arm in an inverted half gallon bottle filled with water from *the same pump.*

EXPERIMENT XIX.

Fahrenheit's Thermometer 69°.

My hand and fore-arm were completely invested with fine varnished silk, which was impervious to air, and introduced into a tub of the same water as above. In a short time, innumerable small bubbles of air were seen by us both, to form at different places on the surface of the silk, and to gradually rise in succession to the top of the water. These bubbles of air presented themselves, and took their flight through the water, in the same manner as we had before observed them to do when the naked arm was immersed in the water.

At the instigation of Dr. Hutchinson, the following experiment was performed, to show more completely the power of a degree of heat equal to that of the skin, to disengage from common water the gases it is impregnated with.

EXPERIMENT XX.

Fahrenheit's Thermometer 70°.

Water, of the standard temperature of the animal body, was passed, during half an hour, through a tube which was fixed in the centre of a large glass globe, previously filled with water directly from the pump. As the warm water was poured into one end of the glass tube, it immediately passed through the middle of the large globe of water, and was discharged through the other end of the tube on the ground. In a few minutes, small globules of air began to form on the external surface of the tube, which was surrounded with the pump-water of its ordinary temperature, and to rise to the top of the globe.

Dr. Hutchinson, though once a believer in the emission of airs by the skin, on perceiving the bubbles of air rising out of the water from the surface of the tube, as I have mentioned, declared he now saw the deception De Milly and Ingenhouz had laboured under. It would be absurd to contend that the air disengaged came out of the pores of the glass tube, and yet, from what experience I have had on the subject, I am disposed to believe such a conclusion would be as just as it would be to assert, that the air which is collected by holding the hand and fore-arm in common water, is discharged by the cutaneous passages. Any inanimate substances, say a stone, a piece of glass or iron, warmer than the water in which they are placed, will, by expanding the airs in the water, by parting with their greater heat, disengage gases as well as the hand and arm. It has been suggested, that water has been objected to as an improper medium for experimenting in; but this objection only holds good against common water, and not against that

which has been well boiled or distilled. All the common gases, excepting carbonic acid, may be collected nearly as well over boiled water as over quicksilver, or any other way. The boiled water certainly absorbs fixed air with great rapidity, but if fixed air should be emitted by the skin, and be immediately combined with the water, such an occurrence can easily be ascertained, by afterwards applying to the water the usual tests for that air.

EXPERIMENT XXI.

Fahrenheit's Thermometer 64° *in the room.*

On the 12th of March, 1809, I introduced my hand, and the greater part of the fore-arm, into a gallon bottle nearly filled with water, which had been boiled for some time, so as to drive off all its loose air. To intercept the communication between the external air and the inside of the bottle, the varnished silk used in the experiments on insensible perspiration was again applied to the neck of the bottle, and about my elbow. In this state I continued during two hours, without either myself or friend being able to discern a single bubble of any kind of air rising through the water.

In this experiment, no one will pretend that the least difficulty existed to the transpiration of air by the skin, if such an office is really executed by it. The cirumstance of the water having been boiled could not have formed an obstacle ; and, I am sure, its temperature was every way conducive to the performance of the natural offices of the skin. On withdrawing my hand, Fahrenheit's thermometer was introduced into the water, when the mercury rose in the tube to 76°. Notwithstanding the common objection to boiled water, I certainly feel safe in concluding, as no air was seen to pass through the

water, none was thrown out by the skin. Admitting the affinity of water for air is increased by having been boiled, yet I cannot think it could be so much strengthened as to incorporate carbonic acid, much less azotic air, with the water, so perfectly, and so directly, as to effectually prevent a single bubble from escaping through the water and being seen. However, that even this last objection shall no longer exist to propagate the least degree of doubt, I will proceed to relate the examination of the water I made after withdrawing my hand and forearm from it. The tests which were applied are,

1. Sulphuric acid,
2. Infusion of litmus,
3. Fresh vegetable leaves.

Lime water is very commonly recommended to detect fixed air in water; but I have not been able to succeed with it as an unequivocal test, having found, when poured into water which had been so much boiled as to dissipate all its loose air, some degree of turbidness ensues; and, if permitted to stand in the vessel an hour or two, a white cloud will present itself at or near the bottom.

I presume, on the two fluids mixing, a small quantity of the lime is precipitated.

Two vials were nearly filled; one with the water I had experimented in, and the other with water from the same pump, which had not been boiled. A small quantity of the sulphuric acid was poured first in the one vial, and then in the other, when copious small globules of air quickly ascended through the fresh water, while in the other no appearance was presented but the mere mixing of the acid with the water. The

infusion of litmus was poured into some of the water in which I had held my hand and fore-arm, and pieces of paper, stained with the litmus, were also introduced into other portions of it; but a red colour, the mark of acidity, was not in either case produced.

In the next place, a small handful of vegetable leaves was introduced into a large tumbler which was filled, and inverted in *the water from which I had just withdrawn my hand and arm*, and a second handful of the same leaves was also introduced into another tumbler which was filled, and inverted *in common pump-water*. These tumblers were exposed, during three hours, at the same time, to the same solar light. They were closely attended to by an assistant and myself. At the close of the time stated, on the top of the common water, we found, that about half an ounce measure of oxygen air was collected, while in the other tumbler, containing the boiled water, in which my hand and arm had been held two hours, only three small globules were perceived, and those adhered to the leaves at the time they were introduced. From the time those globules were seen to rise from the leaves into the top of the tumbler, until the three hours had expired, not *a particle of air* was detached from the water.

From the experiments related under the last department of this investigation, I have concluded, as air is not transpired by the skin while the hand and arm are held in boiled water, or in quicksilver, and as the hand and wrist do not contaminate pure oxygen gas, or atmospheric air, when held in them for hours together, *gases, in all probability, are neither transpired nor absorbed through the surface of the human body.*

Case of Chorea Sancti Viti. In a Letter to the EDITOR *from* Dr. PEACHEY HARRISON.

Harrisonburgh, Rockingham County, Vir. July 15, 1809.

DEAR SIR,

AS you have taken much and very laudable pains to investigate the nature of chorea sancti viti, and to systematize the known facts relative to that disease, I take the liberty of communicating a short notice of a case which has lately fallen under my observation; and I feel the more inclined to submit it to your consideration, because the methodus medendi has, I believe, been conducted upon a plan somewhat novel in the history of chorea; and, perhaps, too, the case may cast some light on the abstruse nature of this disease. But it will be referred to you, sir, to determine whether the method of cure is in correspondence with the theory you have suggested; whether it tends to confirm or subvert it. On this point my mind has not been able to bring itself to any decisive conclusion. The case is this:

Nancy Harrison, aged about 13, of a habit very thin, a complexion pale, delicate, and fair, with eyes prominent, considerable vivacity of spirits, and, in short, with most of the distinctive marks of a highly formed nervous temperament, had, about the 1st of March last, without any previous illness or morbid symptoms of any kind, slight appearances of those peculiar gesticulations which constitute or distinguish the chorea. She had never as yet menstruated, nor had any symptoms, premonitory of that constitutional discharge, been experienced. The symptoms were neither preceded nor accompanied by any pain of the head, back, loins, stomach, or any other part. In short, she had enjoyed her usual health, her

usual vivacity of spirits, and regularity of her mental functions. The muscular agitations were, at first, and for the space of two or three weeks, so slight as to be scarcely discernable ; they were, however, from the first, extended to her head and upper and lower extremities, but were most apparent in her left hand. For a considerable time, her mother supposed her gesticulations to be merely affected, and repeatedly admonished her to discontinue them, to no purpose ; but, on the contrary, they became towards the last of March so distinctly marked, as strongly to arrest the attention of her parents, and awaken their tenderest solicitudes. The physician who usually prescribes for the family was now consulted, and pronounced, without hesitation, the name of the disease, and began to digest, and even to execute, a plan of cure, founded, I believe, on the theory which you have proposed of that disease. The plan, I believe, was intended to include the various means of depletion which might appear to be best accommodated to the strength of the patient, and the state of the symptoms. But the choreatic gesticulations having now become very strongly marked, the novelty of them, and the great apparent sufferings their daughter underwent, created in the minds of her fond parents such indecision, such irresolution, relative to any plan modestly proposed by the diffidence of a physician well acquainted with the uncertain resources of his art with regard to this disease, that the plan was hardly commenced until it was exchanged for one more confidently proposed by a *good woman*, living at a few miles distance, who, having heard of the afflictions of the family, was so obliging as to resign the cares of domestic life, for a time, and take upon herself the important functions of a physician. She found the patient labouring under precisely such symptoms as she herself had experienced when a girl, and which had yielded to the prescriptions of some eminent physician with whom she had

the happiness to be acquainted; and, indeed, the remedies themselves, which this doctress proposed, were far from being empirical; but the *manner* in which they were to be taken was somewhat so. The cold bath was to be used twice a day, in some certain form, which we shall omit to detail. One or two doses a day of the barks, and every third day three pills of asafœtida, containing from six to nine grains. This method was adopted and pursued, without much variation or remission, for five or six weeks; the muscular agitations, however, became, upon the whole, stronger, more confirmed, and more distressing, except that after the adoption of these remedies it was believed that her sleep was less disturbed. But at no other time than when asleep, was there one moment of intermission in the gesticulations; and these were so violent as to shuffle out a pair of shoes in a little time, to loosen her teeth, and to make it necessary to fix soft cushions in the palms of her hands, to preserve them from injury. It was with the utmost difficulty she could take food, and became entirely unable to walk, owing to the extent and strength of the spasmodic contractions of the muscles of the lower extremities; articulation, from the irregular motions of the tongue and under jaw, became also very indistinct.

The case had now assumed a very serious and even alarming aspect, and called for speedy assistance if that were to be had. The attending physician proposed to try other remedies than those which had been tried so long in vain; but, not being proposed with that bold effrontery which alone can overcome that apathy and irresolution which some minds unhappily possess, they were never submitted to, but the whole routine of neighbourhood panaceas, some of which were shocking to common sense, and a scandal to human reason, were eagerly adopted.

This was the state of things, when, some time in the last of May, information was received, through a very confidential channel, that a certain Dr. Sullensteine*, of chivalrous fame

* This Dr. Sullensteine assumes to himself the name of the Turkey doctor, and seems to plume himself greatly on the circumstance of his having been, as he says, bred in Turkey. He seems to think, or wishes it to be thought of him (and in this he shows himself to be an adept in human nature), that this circumstance is greatly favourable to the opinion of his having medical skill. We know what a magical effect antiquity has on all human labours, and that remoteness of *place* has nearly the same deceptive influence on the opinions of mankind with remoteness of *time*. Whatever has been transacted in a distant time or place, has, with men, a greater interest and a greater value. Hence we read with greater avidity the history of ancient and distant nations than that of modern times; hence, too, we are disposed to set a higher value upon the products of the arts and sciences of other nations than upon those of our own. From this principle in human nature, perhaps, springs also the popular opinion, that the art of medicine is more successfully practised amongst remote, barbarous, and savage nations, than amongst ourselves. Hence those itinerant doctors, who have professed to derive their skill from the North American savages, have been greatly resorted to, for a time at least, and the wonders they have wrought have been propagated, with great zeal, through extensive sections of country. Indeed, it seems to be a universal disposition of human nature, to view with a regard somewhat superstitious, whatever is too remote for the investigation of our senses, or too dark for the comprehension of reason.

Of this principle Dr. Sullensteine very successfully avails himself, by laying the scene of his medical education at a greater distance, and in a country more barbarous. It is more than probable, however, that he was never in Turkey, much less bred there. His dialect appears to be that of a Prussian; but, whatever has been his country, it is very certain he never received a medical education any where: but we will do him the justice to allow, that he possesses those qualities which render him pre-eminently fitted for empirical practice: a body at once active and patient of labour; a mind destitute of all science; but, in the same degree that it is ignorant, it is bold, assuming, confident; discerning enough, however, to know when to conceal his ignorance, and when to set the multitude agape by a pompous recital of the many wonders the art of medicine has achieved when directed by his unerring hand. He seems to

in medicine, possessed great skill in the management of all kinds of *fits*, and especially of that form exhibited in this case. Now, as the skill of all the *christian* doctors in the neighbourhood had been exhausted in this case, it could not be blameworthy to call in the strength of even a *mussulman* to subdue this gigantic disease. Dr. Sullensteine is sent for; he comes;

take a pride in undertaking whatever diseases other physicians have declined, or have not succeeded in curing. In short, no disease is so difficult or so inveterate, but he will confidently undertake its cure: and he always attempts to accomplish his undertakings with the *Sampsons* of the materia medica, administered with a bold hand, and with much indifference whether life or death is the result: and it cannot be denied, and it is what every physician would expect, that he sometimes effects great cures. He is one of those doctors who can pursue, with the scalpel, proteiform disease into its most retired situations. Consumptions, so fame announces, have yielded, without a struggle, to the dissecting knife in his hand, in the extirpation of large portions of diseased liver! and, in a case, the subject of which lives in my own neighbourhood, and of which I had the narrative from her own mouth, he was enabled, by superior craft and circumvention, to *catch* and extirpate a living monster, that had for a long time kept its retreat under the knee-pan of the unhappy subject, but which occasionally sallied out of its hiding-place, and ventured a small distance up the thigh. This monster, fell as the hydra of Lerna, had, for a considerable time, eluded the consummate skill and address of Drs. Huter and Miller, as famous in the annals of medicine of our country, as Don Quixote and his squire formerly were in the annals of chivalry: whenever it was found abroad from its lurking-place, before they had the good fortune to seize upon it, it had escaped their grasp. Dr. Sullensteine was, however, better versed in chirurgical tactics, and took the miscreant monster by a coup de main. Its appearance, was, however, too horrid for public inspection; there was no knowing what mischief it would do if permitted to go at large: it was thought most prudent that it be overwhelmed in the flames! But, although, previously to the operation, this medical chevalier had inspired the patient and her father with the utmost confidence in the art, which he solemnly averred he possessed, of *staunching* blood, yet the hemorrhage went on in spite of all his arts, until evanescent life quivered on the pallid lips of the patient! from mere spoliation of blood, life hung in doubtful suspense for several weeks!

"That's just a swatch o' Hornbook's way."

he examines the patient; the case is quite familiar, and no doubt is entertained but it will yield to his remedies in about fifteen days. His plan is proposed; there is some hesitation as to its adoption; it is altogether immaterial to him. If they do not choose to submit to his plan, after putting him to so much trouble, and with a certain prospect of a cure, upon the event of submission, he could return home again.

You may easily guess that he was permitted to go on. Seven blisters are prepared by mixing the powdered flies with fermenting leaven. One is applied between the shoulders, two on each arm, and one on each leg, all pretty large and strong; insomuch that large vesications were produced in about eight hours; but the size of the vesications were not determined by the size of the blisters: for the agitations of the patient rendered it impracticable to confine them (especially that between the shoulders) to a particular place.

Your imagination can supply you with a conception of the patient's sufferings, or rather tortures, during the operations of the blisters, better than any description of mine. Suffice it to say, that the tortures of the vesicatories had nearly dislodged the immortal tenant!

The evening of the next day she had blood let from the foot, to a small amount, and ordered to be bled in a few days again, but this was neglected; was to take some aloetic pills every few days, but these did not operate; took twenty drops of camphorated spirits thrice a day; and, after the vesications dried up, her limbs were to be embrocated with a liniment composed of sweet oil and spirits of turpentine, *with a few red ants*. Immediately after the blisters began to discharge freely, the gesticulations began obviously to abate. Her skin,

which had heretofore been dry, and, it was thought, somewhat thickened, now became soft and perspirable, and articulation began to be more distinct. The blisters continued open for several days, nor, as it was apprehended, did the symptoms recur upon the drying up of the blisters, but turned gradually more favourable; and, at this time, the gesticulations have nearly ceased, and, in all respects, her health is re-establishing as rapidly as could be expected.

I am, respectfully yours,

PEACHEY HARRISON.

Dr. J. R. Coxe.

A Description of the Birth and Form of a Human Monster. Communicated by JOSEPH KLAPP, *M. D.*

DEAR SIR,

THE bearer will hand you an account of one of the most curious phenomena which perhaps ever fell under the notice of the American faculty; and, if you should think proper, you may give it a place in one of the future numbers of your Museum. The subject of the communication is a human monster, of which a black woman, in the month of March or April last, was delivered, in some part of Bucks county, in this state. The circumstance fell under my notice about two months subsequent to the occurrence of the birth. So far as I can learn, the parents came to this city at the instigation or advice of some ignorant persons, with a view of making money by exhibiting the offspring as a show. A little experience, however, I believe, served to convince them that their undertaking would be attended with more expence than profit. Promiscuous or common spectators, from whom the

proprietors of the many curious objects of public show chiefly receive encouragement, would, I should suppose, avoid rather than court the observance of such an unpleasant spectacle.

After they had been in the city several days, the husband called on me to visit his wife, and then informed me of his having at home this curious offspring. The brief relation he at this time gave me, was such as could not fail to excite my curiosity: I requested him to return, and I would follow him shortly.

On visiting the woman, whom I found much indisposed, her husband placed before me, on the table, unquestionably the most extraordinary lusus naturæ I ever beheld, and what appeared almost equal in singularity to the human monsters which we find described by the marvellous writers of former times. It consisted of two infants, intimately united to each other from the umbilical region up to their head; possessing distinct upper and lower extremities, and a body, neck, and head, common to both children. The head is of an oval figure when viewed laterally, but viewed anteriorly, and including the forehead, it seems somewhat triangular. They are of the same size and length. As they are suspended in the large bottle of spirits of wine, their shoulders, elbows, hands, knees, and feet are placed opposite to each other, and nearly in contact. This disposition of parts is owing to their being applied or united to each other at their breast and anterior part of their abdomen. Their hips and backs are of the usual form, and their bodies are gently inclined towards each other, while their abdomens at the umbilicus unite, and present a smooth uniform surface, covered with a common cuticle. In the centre of the abdomen, common to them both, the funis, in a perpendicular direction, penetrates the cavity so as to be situated at

an equal distance from the proper body of each child. From the umbilical region downwards, they are remarkably well formed; so much so indeed as to give them more appearance of beauty or symmetry, than is commonly perceivable in new-born infants. Their joints are well formed, and their limbs, both upper and lower, with a single exception, are perfectly usual and well proportioned. The exception alluded to consists in there being two thumbs to one of the hands of one of the children. The forehead, and the face generally, is flattened and extended; the malar, or cheek-bones, are further apart than usual. The mouth being open exhibits a perfect tongue and palate. The eye-lids are closed; and, on particular examination, were found to cover well-formed eyes. The general colour of the skin is of a lightish cast; but some parts of it, such as on the hands, the middle of the anterior part of the abdomen, the inner side of the thighs, and the inner side of the arms, the colour is of a reddish brown. The head is covered with a fine kind of dark-coloured hair, not much unlike, or probably would not have been much unlike, that of the mother. Though a coloured woman, yet she has pretty long, distinct, or straight hair, different from the wool of Africans generally. The neck and shoulders are covered with a fine kind of down or fur, of the same colour with that of the head. Under and about the ears, it is much longer and more distinct than at other places. The length of the down or hair is greater about one ear than the other. I have not measured it at this place, but I dare say it is half an inch in length.

The termination of the rectum, and the sexual organs, have a natural appearance. They are both females. On placing the monster in an accurate pair of scales, it appeared that its weight was two pounds and twelve ounces. Its length is four-

teen inches; the breadth of the shoulders is four inches; the length of the arms is six inches and a quarter; and that of the legs six inches. The circumference of the body, at the shoulders, is eleven inches, and that of the head twelve inches and a quarter.

It will be unnecessary to introduce, in this place, a more minute description, as the subjoined plate, containing two of the most interesting views of the object, with explanatory references to the principal parts, will afford ample, and every necessary information. Being informed that Dr. Beatty, of Bucks county, had attended the woman during her confinement, I addressed to him the following letter, to solicit from him such information as his attendance on her had enabled him to collect.

Southwark, June, 1809.

Sir,

As I have not the pleasure of a personal acquaintance with you, it might be considered rather presuming to address a few lines to you on any familiar or common occasion; but, as the purport of my letter is of a nature interesting to science, I trust you will excuse the liberty I have taken with you.

A few days since, a coloured man, lately from your neighbourhood, who calls himself Silas Hunt, left at my house, at my request, a human monster, unquestionably the most singular which has ever transpired to the knowledge of the faculty of this country. So wide a departure from the common form could never have been anticipated, though past experience had supplied us with many more instances than it has done, of Nature's sportive power. He says his wife is the mother of

this production, and that you had the goodness to attend her during the indisposition which followed her delivery.

My object, then, is to solicit from you such information relative to the circumstance, as your attendance on the woman may have enabled you to obtain. I have conversed with her frequently concerning it, and she has given me much curious and interesting information; yet your attestation and account of the event is, notwithstanding, very desirable. I have had a very accurate drawing made of the monster, and propose, at some convenient time, perhaps in the course of a few months, to publish a brief relation of the fact, with the representation of the object.

I assure you, sir, any account which you may find convenient and agreeable to furnish me with, will be very thankfully received.

With respect, I am
Your humble servant,
JOSEPH KLAPP.

Dr. Reading Beatty.

To this letter I was favoured with the following reply, which I have ventured to insert verbatim, in the present communication, without having asked permission from its author. I beg he will excuse the freedom, as I am convinced the interesting matter it contains, is much better detailed in his own language than it would have been in mine.

SIR,

I ought to apologize to you for the apparent neglect your letter has received. I assure you, it was entirely unintentional. From the time your communication arrived, I have been con-

siderably engaged in professional duties, which has left me little leisure for affording you the desired information. Another motive for delay was, that I wished to have some conversation with the female who assisted Sylvia Hunt at the time of her delivery. She is not a professed midwife, but has occasionally afforded her services in that way.

The amount of her information is, that, while the husband was gone for the nearest physician, Sylvia's pains having increased much in force and frequency, she thought it necessary to examine her situation. The membranes were still whole, but through them, in the intervals of pain, she could feel one of the hands. As soon as the water was discharged, she again examined; and, finding the arm likely to present, she, with a considerable degree of resolution, and some judgment, put back the arm, and searched for the feet. In doing this, she found more feet than could belong to one child; and, concluding there must be twins, she endeavoured to keep back one set of feet while she drew forward the other. But finding that drawing upon what she supposed one child invariably brought on the other, she determined to find the cause; and, extending the hand still further, she discovered the junction. She then grasped the whole of the feet in her hand, brought them down, and happily freed the mother from her burden. She says that the monster was born alive, and breathed at intervals, she thinks as long as fifteen minutes.

Some days after delivery, I cannot say how long, I was sent for. I found her labouring under a considerable degree of fever, and a greater discharge of the lochia than was customary. In a few days, however, she was relieved from these symptoms, and I think that in a period not much longer than usual

she recovered. This, I think, includes the whole information I can give you respecting this uncommon case.

Sylvia Hunt has been occasionally under my care for more than three years. The complaint to which she has been most subject, was a uterine hæmorrhage. The discharge was, at times, very profuse, and always tedious, and with difficulty yielded to medicine. The common applications, both internal and external, were resorted to, particularly the sacchar. saturn. which was pretty freely given; and although the relief was not immediate, yet the good effects were very visible. The last time of my attendance upon her in that complaint, was in the month of September last; and, to the best of my recollection (for I have kept no minute of the case), she was not entirely free from it before the latter end of the month. She was delivered of the monster, I think, some time in April, by which it appears that she probably carried it only seven months.

One circumstance more respecting Sylvia, which is rather out of the common road of such things, I must relate.

In February, 1808, I attended upon her during one of her labours, when she was delivered of twins. The child first born, from the colour of the skin and hair, was such as might be expected from such parents. The second child was extremely small, came into the world entirely enveloped in the membranes, and did not see the light till I released the little prisoner. This last child was considerably different from the former, had a much fairer skin, and the hair light and straight. Both the children died when but a few days old.

I am much pleased to hear that you have had an accurate drawing made of the monster, and shall be much gratified in

seeing the draught, and perusing your publication respecting it. When I visited the mother they had put the object into a pitcher, and covered it with spirits. From the size of the pitcher the child could not be readily seen, and I had a very imperfect view of it.

I am sorry that the information given above is not more complete and satisfactory than I am afraid you will find it. Such as it is, I am happy in communicating it to you.

Your's, very respectfully,

READING BEATTY.

Fallsington, August 12, 1809.

DESCRIPTION OF THE PLATE.

Fig. 1. Affords an accurate, and the most interesting view of the monster.

A. A. The lateral protuberances of the common head, apparently formed by the occipital and posterior portions of the parietal bones, manifesting an original effort of nature to construct two distinct heads. This idea derives support from the opposite direction the hair lies in from the centre or middle point of each convex projection; some of it running over the crown of the head, and some towards the neck.

B. The forehead, very much flattened.

C. The face, of an unusual length.

D. D. The cheek bones, placed at an unnatural distance from each other.

E. A portion of the funis umbilicalis included in a ligature.

F. A depression of the common integuments between the anterior parts of the children. Whether there is a sternum for each chest or a common one to both, could not be positively ascertained by an external examination with the fingers; but I am inclined to suspect, there may be an imperfect and separate substitute for a sternum in both cases.

G. Represents the long hair which is spread over the neck and shoulders. This hair is longer about the neck, and near the ears, than at any other place.

H. H. H. H. H, &c. Exhibit the joints and limbs in their usual state, and well proportioned.

I. I. The eye-lids closed, and covering well-formed organs of vision.

Fig. 2. Shows the figure and appearance of the head, from a posterior or lateral view.

A. A long aperture or sinus which proceeds into the head.

B. B. Cartilaginous and fleshy substances which bear a great resemblance to ears.

C. C. The neck and shoulders.

D. The umbilical cord.

E. Representing two thumbs on one of the hands, both perfect in joints and form.

Fig. 1.

..2.

F. A part of the fore-arm and hand, omitted, as not necessary to be delineated.

On the Principle of Animation in the Human Body, by THOMAS D. MITCHELL. *In a Letter to the* EDITOR.

DEAR SIR,

THE following imperfect observations were occasioned by the perusal of an Inaugural Dissertation on the Principle of Animation, by Dr. Macrery, of Delaware. If the few remarks I am about to make be worthy your attention, you will oblige me by inserting them in the Medical Museum under your direction.

As all nature owes its origin to some being, some great first cause, which created all things, it is of little consequence by what name we characterize this great being, this infinite power. If we style him God, the great first cause, the alpha and omega, or the governor of the universe, we intend to signify the same personage, the creator and preserver of all things, the almighty controller of events. Then, as all things owe their existence to this great power, they can only be considered as effects derived from this great cause. Therefore, every event in nature, every preternatural occurrence in the course of things, be the instrument which exerts them what it may, the grand principle of action can spring only from this first cause.

My intention here is to consider the principle of animation in the human body; the reader may have already anticipated the conclusion which follows: those who believe that an infi-

nite power created the world, cannot long hesitate to acknowledge the ability of that power to form the creature man. Though this be the noblest of the works of God, yet it excels the rest only in quality, not in degree. Then, if man be a created being, his powers are undoubtedly under the controul of his Creator; and, if man be thus subjected, then is the principle of life which supports him wholly derived from his Creator.

But some are ready to argue that this kind of reasoning is unphilosophical. I am ready to grant that the reception of this animating spark does not come to us from the hand of the Creator in a visible manner; but, although this be true, will any one deny the existence of an animating principle? and, if there be an animating principle, if we are sensible of its existence, shall we deny that it is received from the Creator, merely because he chuses to grant it in a mysterious manner? If we declare as false, things which are really true, because we cannot comprehend them, we might at once deny the existence of man: but sound logic teaches us otherwise. The principle of animation, the vital spark, is the gift of the great first Cause indirectly, through the medium of a wonderful agent pervading universal nature, and known by the different names of oxygen gas, dephlogisticated air, vital air, &c. This is the great instrument by which the wise Creator chuses to effect the principle of animation in the human body; and this, together with its *modus operandi*, shall now claim our attention. One of the distinguishing proofs of the existence of a first Cause, is afforded by the manner in which this vivifying principle is distributed through the universe: no clime is destitute of it, and all share its blessings nearly in an equal degree. It is unnecessary to speak of the manner in which it is afforded; we know it is diffused far and wide, and every living being shares

its bounty. Our business is to attempt to explain the manner in which it operates as a living principle in the human body, and is productive of irritability in it. To suppose a body capable of irritability*, we must first consider that body to be possessed of life; therefore life precedes irritability, and oxygen, being the principle of animation, is, consequently, the source of irritability: this we shall endeavour to prove.

Place a body in a quantity of the air of the atmosphere deprived of oxygen, and it will certainly die. What is the cause of its death? It is deprived of the principle which before animated it. It is of no importance here to examine the changes in the body in consequence of the absence of the vital principle; it is sufficient to know that its absence occasioned death. The body is now deprived of its former irritability, and the reason is obvious: that principle which was the cause of irritability now ceases to act; and, of course, where there is no exciting cause, there is no effect produced. This certainly proves that the presence of oxygen is necessary to life. But some have objected to oxygen as the cause of irritability. Dr. Macrery is among the number of those, and, in his dissertation, he asks, whether the effect produced in the living body by stimuli applied, such as pieces of broken glass, points of pins, &c. is owing to a combination of the oxygen of the body with these stimuli; and if so, in what manner do they unite? We do not pretend to assert that a chemical affinity takes place between the oxygen of the body and the various stimuli that may be applied, nor would we wish to account for this circumstance in any other way than through the medium of actual cause and effect. We have already shown that a body with-

* By irritability in the body we mean, throughout this essay, a sensibility of pain, arising from stimuli applied.

out oyygen will die, and that death and irritability in the same body cannot be; and, therefore, that a body to be sensible to stimuli applied, must be possessed of life, and consequently of oxygen; so that oxygen is the cause of irritability. It is granted that oxygen does not act directly with the stimuli applied to the body so as to produce irritability; but this does not argue that oxygen is not the efficient cause of that irritability; the action of oxygen is through a medium, and that medium is the body. By the action of this principle, the body acted upon is rendered capable of irritability; and, as it was before said, where this principle does not act, its effects cannot follow. When violent stimuli are applied to the body, these act as the remote cause, oxygen as the proximate cause; injury of the part is the remote effect, and sensation of the injury, or irritability, is the proximate effect.

Dr. Macrery further asks, does volition combine with the oxygen of our muscles when we chuse to walk? The doctor certainly was not aware of the objections which might be adduced against his proposition, when he wrote as he did. Are not the muscles exerted when volition or the will is not called into action? Does not the raving maniac walk with as much ease as he who enjoys the blessings of sense and reason? The will is a passive instrument in this case; or, if it were an agent in the action of walking, why would not the decrepid and lame, whose intellectual faculties are unimpaired, travel with as much facility as those who enjoy health and regularity in form and stature? Will the doctor declare that the action of the muscles cannot be exerted, independent of the agency of the will? He, himself, is a living witness to the contrary; therefore his proposition involves nothing of importance with respect to the effects of oxygen in the human body.

Authors have said, that if oxygen be the cause of irritability, how is it that irritability is so completely marked in the fœtal state, where the action of oxygen is so little exerted? The being in the womb, from its first appearance there, until birth, being so intimately connected with its mother, must derive its life from her, through the medium of the oxygen which she imbibes. That intimacy which subsists between the embryo and its mother, renders it a part of the mother, and of course dependent on her for subsistence while in its present state. If the oxygen of the parent be the cause of her irritability (which has been proven already), and as every part of the body is equally liable to irritability, in consequence of the diffusion of oxygen, so the child, being a part of the parent, must partake of the irritability of its parent, and, of course, must be under the effects of oxygen; and, on account of the extreme delicacy of its texture, a proportionally lesser degree of oxygen is capable of producing a proportionally greater degree of irritability than in the mother; and this is the reason why, in the fœtal state, irritability is greater, though the quantity of oxygen imbibed be less.

Finally, oxygen has been objected to as the principle of animation in the human body, because that, notwithstanding its abundance, death is inevitable. The limits of this essay prevent that enlarged view which we might otherwise take of this subject. All know that death is certain. It is a known law of Nature, and of Nature's God, that all must die: but, as the principle of animation was bestowed by the first Cause, through a medium, so also is it removed through a medium. This is effected in various ways, as in suicide, sudden death, intemperance, by chance or design, and protracted severe illness: all these lead to one conclusion. Because oxygen is the principle of animation, does it of course follow that this princi-

ple cannot be counteracted in its effects? If a flourishing tree be cut down as fast as it gains a considerable height, will the vegetating principle which nourishes it be able to raise it above that height? may not the tree be completely cut down and so destroyed that the principle of vegetation cannot restore it? Thus in the human body. If the system be, by any means whatever, disorganized or depraved, it follows, that the force of the action of the animating principle is destroyed, that it cannot exert its full effect. Therefore, the greater the depravation or disorganization of the system (whether suddenly or progressively), by so much the more is the animating principle counteracted in its effects; this principle of action continuing the same, and the system of re-action decreasing until death.

I am yours, &c.

THOMAS D. MITCHELL.

Dr. J. R. Coxe.

On the Method of Preparing and Administering Mercurial Gas in Phthisis Pulmonalis, by Dr. P. W. LITTLE. *In a Letter to the* EDITOR.

Harrisburg, September 13th, 1809.

SIR,

INCLOSED, I send you a communication from Dr. Peter W. Little, on the method of preparing and administering the mercurial gas in phthisis pulmonalis. It is necessary to observe, that these observations were drawn from the doctor in consequence of my writing him on the subject, and requesting the contained information. My wish to be informed was excited by the notice which Dr. Little gave of his administering the mercurial vapour, in his letter to Dr. Rush, which appeared in the 5th volume of the Philadelphia Medical Mu-

seum. I feel it my duty to apologize to the doctor for my negligence in forwarding his communication, and depriving the public of his ingenious and useful observations. My omission did not arise from any want of respect for the importance of the subject, but I wished to forward, at the same time, a case of phthisis which came under my own notice; the successful issue of which I owe, in a considerable degree, to the above-mentioned letter to Dr. Rush. My engagements have prevented me, heretofore, from the attainment of that object; but I have now the pleasure of accompanying the inclosed with the case, the destination of which is submitted to your sound discretion by, sir,

Your humble servant,

SAMUEL AGNEW.

Dr. John R. Coxe.

Mercersburg, May 20th, 1809.

DEAR SIR,

I am very sorry it was not in my power to answer your letter of the 17th April, with which I was honoured, sooner; but be assured, sir, that any information I am capable of giving relative to that now prevalent disease (phthisis pulmonalis), shall be communicated with a great deal of pleasure and satisfaction. At the time I had the honour of making the communication to our learned professor Rush, I had (and still have) it in contemplation to make some experiments, with a view to discover a more ready and perfect mode of administering mercury in a state of vapour or gas than I had then made use of; consequently omitted mentioning the process used. But, from my necessary attention to an extensive and laborious practice, I have never yet, to my satisfaction, been able to complete the experiments contemplated. But as you requested me to inform you the process used to administer mercury in a

state of volatilization or gas, and as I may not for some time have an opportunity of improving this method of administering mercury; and further, as you have a phthisical patient under your care, upon whom you will have an opportunity of improving and perfecting the method, I shall describe the imperfect process used in the cases communicated to the doctor; and, should it prove equally successful with you, and save the life of but one unhappy individual affected with that volcanic disease, I shall feel myself amply compensated for all the anxiety and diffidence with which I make this communication.

Hydrargyrus, in a degree of heat in which it would rise easily in vapour, imbibes pure air and becomes a red calx. This aptitude (if I may be allowed the expression) of mercury to imbibe *air*, and thereby becoming a calx, appeared at first an insuperable obstacle to this mode of administering it; but, upon further reflection, I was led to believe, that if mercury was triturated with a substance equally volatile, and reduced to extreme division of its globules, thereby exposing a greater surface to the action of heat, it might be conveyed into the lungs in a state of vapour: I therefore triturated hydrargyrus with gum. myrrhæ, until its globules entirely disappeared, and had the following machine made for administering it, viz.: a hand-stove made of sheet-iron, about 7 inches square and 5 high, in the top-plate of which I had a circular hole cut large enough to admit a tin of 10 inches in circumference, and 4 high. In this I placed a phial surrounded with sand. To the mouth of the phial I had a flexible tube fixed, about 6 inches long, and fitted with an ivory mouth-piece at the other end. This tube was fitted on the mouth of the phial, so as to be taken off at pleasure. This flexible tube is made after the manner of that described by Bell of Mudge's inhaler. The sides of the stove were perforated with holes, in order to ad-

mit air to keep up the fire, which is made of charcoal. To this stove I had four feet and a handle fixed, for convenience of standing and handling. Hydrargyrus may also be combined with gum olibanum, benzoin, Venice turpentine; or, instead of these, cinnabar may be used, and conveyed into the lungs by using the above machine. The vapour or smoke arising from cinnabar thrown upon live coals, has long since been recommended by Dr. Turner and others, as a very useful and efficacious remedy for drying and healing venereal ulcers and sores, particularly those of the nose, mouth, and throat. A solution of cor. sub. of mercury, in water, may be used with great advantage, by inhaling the vapour when heated*. Mudge's inhaler would be the most convenient, but, if that cannot be had, a tin cup and funnel may be substituted.

I direct my patients to inhale the vapour frequently in the 24 hours when I wish to salivate with it, and when their mouth becomes affected, to use it less frequently. I have frequently thought that we but too often fail in perfecting a cure in this disease, by not keeping up the ptyalism long enough to restore the healthy action of the lungs. I therefore always advise my patients to keep up the effects of the mercury for a considerable time, but in a moderate degree. Calomel may also be conveyed into the lungs by triturating it with myrrh, olibanum, benzoin, &c. Precipitate, and almost every preparation of mercury, by triturating it with a substance equally volatile, and using the described machine, &c. The uncertainty of the dose may, with some other objections, be brought against this mode of exhibiting mercury. But are not all doses of this herculean remedy, given even in the usual and

* It is presumable that any advantage arising from this, must be solely derived from the aqueous vapours, as corrosive sublimate does not rise with this heat; and hence water alone may be substituted.—Editor.

long-customary forms, in some degree uncertain? How frequently do we find but a single dose of 10 grains act as a vomit, a purge, and produce a salivation; when, at another time, in the same person, 200 grains, given at different times, will not salivate!

The effects of mercury upon the human body are now so well known and familiar to every practitioner of medicine, that I hope the uncertainty of dose will not be urged to the general adoption of this mode of giving it in consumption. You ask, have I an opinion that mercury may be introduced into the system in this way when it fails in other ways? or that it will more effectually relieve in this than the usual plan? Few subjects in the science of medicine have excited more attention, or a greater degree of controversy among physicians, than the modus operandi of mercury upon the human body. A question so interesting, and of such importance, in a practical point of view, could not fail exciting attention; and accordingly it has been the subject of much theoretical dispute and speculation. Almost all, however, who have written upon this subject, have agreed in considering it a stimulant, and acting specifically upon the glandular system, in whatever form applied. Consequently, if we consider the extent of the glandular system exposed to its immediate and direct action, when received by the mouth in a state of volatilization, we must necessarily conclude that it is the readiest way of exciting the system into action, and that, when we consider the associated actions subsisting between the vessels of the mouth and those of the stomach, &c. it will not be so liable to run off by the bowels as when given in the usual way.

My worthy and ingenious friend, Dr. Elijah Griffiths, of Philadelphia, told me, that when he failed exciting a ptyalism by giving mercury in the usual way, he always suc-

ceeded in producing a speedy salivation by making his patients breathe the vapour arising from cinnabar thrown upon live coals. In the fourth volume of the Edinburgh Medical Essays, Mr. Hill mentions a case, where the vapour arising from only half a dram of cinnabar drawn up into the mouth and nose, raised a salivation in the space of three hours. We are also told by the same gentleman (as mentioned in my letter to Dr. Coxe, and by him published in the Medical Museum), of a salivation being excited in three hours, by inhaling the mercurial vapour which arose from some mercury which was suffered to stand upon a heated stove with a view of purifying it. We have also the authority of Chaptal, of a salivation being produced merely by inhaling oxygen gas, which was obtained from red precip. From my own experience, and the above facts, there remains no doubt in my mind, but mercury may be introduced into the system in this way when it fails in other ways, and the system affected and excited into action with more certainty, and quicker, than any way yet proposed and administered. That it will more effectually relieve in this than the usual plan, I have also no doubt, particularly when we consider that the lungs are the seat of disease ; and that mercury, thus administered, acts as a topical application to the part diseased. From the situation of the lungs, we have no way of applying topical remedies to them, but in a state of volatilization ; and I have already mentioned, that the vapour of cinnabar has been used with much advantage in drying and healing venereal sores and ulcers situated in the nose, mouth, and throat. Would not this salutary effect extend to ulcers, &c. of the lungs, if properly conveyed into them. Physicians, until very lately, paid too little attention to this mode of applying medicine to the lungs in a diseased and consumptive state. The beneficial effects of topical applications to ulcers, &c. not kindly disposed to heal, are daily experienced ; and, as there is no way (as already observed) of ap-

plying external applications to the ulcers, &c. of the lungs, than in a state of volatilization, it must appear obvious, that it will more effectually relieve in this than the usual plan of treating the consumption.

The difficulty, and the cause of our failing but too frequently, in curing pulmonary consumption, arises first, from the constant action of the lungs. The pernicious and injurious effects of walking, &c. in ulcers of the legs, &c. are daily evinced. But as the constant action of the lungs is indispensable with life, this can never be remedied. Secondly, from the constant admission and action of atmospheric air upon the parts diseased. Hitherto, too little attention has been paid to the temperature and air of the rooms of those labouring under phthisis pulmonalis. The insalutary effects of atmospheric air in external (obvious) wounds, are daily experienced, and too well known to you to require proof. It is also well known to you, that if large ulcers are exposed but for a short time to air, a hectic fever ensues. The action and admission of air to the ulcers of the lungs, must be equally pernicious, insalutary, and the cause of the hectic state of the consumptive. Do not the hectic symptoms always succeed the purulent and ulcerated discharge from the lungs? I have considered the due regulation of the temperature and air of the apartments of consumptive patients, of primary importance in the treatment of that disease, and as meriting our constant attention. The temperature of the room ought always to be such as is agreeable to the feelings of the patient, and as uniform as possible. Impregnating the atmosphere of the room of consumptive patients with mercury, will be found of great service. Hence I cannot but observe, that I disapprove of consumptive patients performing journies on horseback, until the healthy action of the lungs is restored; because they are then exposed to all the vicissitudes

of weather and change of air. In doing this I am fully aware I oppose the opinions of most authors who have written upon this subject, particularly the immortal Sydenham, who says that riding on horseback will as certainly cure the consumption as bark the intermittent fever. Few instances, however, confirm this opinion; and, where it has been of service, I believe it to be only in those cases in which extreme debility of the system continued after the healthy action of the lungs was restored, or where the lungs were never ulcerated. For exercise, I always direct the use of the swing and the rocking chair, until the lungs have assumed their healthy action. I then advise exercise on horseback, increasing the distance with the increasing strength of the patient: but only in pleasant days, and when the temperature of the atmosphere is dry and uniform. As fire has a tendency to rarify air and render it more salutary and agreeable to phthisical patients, I generally recommend the use of fire, and confining themselves to the room in damp weather and days during the summer season.

As I have already far exceeded the limits I set to this imperfect and now prolix letter, I shall merely observe, that the patients alluded to were evidently in the last stage of phthisis pulmonalis. They were much emaciated, pulse small and frequent, beating from 120 to 130 in a minute; febrile exacerbations, profuse night sweats, and a great discharge of thick pus, with extreme difficult and laborious breathing.

The medicines used were principally mercury, digital. opium, episp. &c. prescribed according to the symptoms and state of the system. I have never found any medicine act so powerfully and beneficial in restraining the profuse night sweats as the following preparation: Take sulph. of iron, acet. of

lead, ana ℥ij. French brandy 1 ℔. digest for 24 hours, &c.; of this I gave from 5 to 40 drops twice a day. I have generally added some laudanum at night. I was led to prescribe sac. saturni from its powerful effects in checking hæmorrhages, and I was happy to find it answer my most sanguine expectations. In the place of salt of iron, ipecacuanha may be added with great advantage, so as to excite a slight nausea, &c.

I hope, my dear sir, you will pardon the prolixity of this letter. Should you think it worthy a place in the Medical Museum, you are at liberty to forward it to Dr. Coxe. I do not by any means consider the process used to administer mercury in a state of vapour or volatilization as complete or perfected; it is not yet matured, and but an imperfect child. All I expect, or can hope for, is, that it may prove an exciting cause to some one more capable of suggesting a better and more perfect method of exhibiting it in a state of gas.

I remain,

Your friend and humble servant,

PETER W. LITTLE.

Dr. Samuel Agnew.

A Case of Phthisis Pulmonalis successfully treated by the Acetate of Lead and Digitalis. By Dr. SAMUEL AGNEW.

THE history of the present case is given as it was committed to my common-place book during my attendance on the patient, J. T. a young man about 25 years of age, of a florid complexion.

He had been affected, about the beginning of November, 1808, with a quartan fever, which continued several weeks,

but did not prevent him from his usual labour the intermediate days. Having a strong opposition to the use of medicine, he neglected himself, under the delusive hope that the fever would soon leave him. At length, finding no prospect of relief, he was induced, by some of his good neighbours, to take a celebrated (and no doubt a useful) nostrum for the ague, viz. a composition of bark, snake-root, and crem. tartar. I had previously induced him to be bled and take an emetic. The medicine checked the fever, but did not produce that complete restoration to strength which was necessary to secure a continuance of health. However, he remained free of any marked state of fever during the greatest part of the winter.

On the 16th of April, 1809, he complained to me of bad health, and requested something to be done. He alleged, he had received his present indisposition from a strain in making a violent exertion at lifting a heavy weight, 5 or 6 days previously. He complained of a severe pain across his abdomen and lumbar region, general weakness, some pain in the head, and irregular fever. I took some blood, applied a strengthening plaster, and prescribed the use of bals. copaib. Under this treatment he became better, in a few days, of the pain in the abdomen and back, with some acquisition of strength: but his breast now became, unfortunately, the seat of disease. He complained of some pain in the breast, hard dry cough, an irregular fever, with exacerbations and want of appetite.

On the 20th, I gave him a cathartic, and some antimonial powders to follow the use of the purgative, and restricted him in his diet. The medicines operated well: complained much of the antimonial powder producing severe sickness. He felt

easier ; pain abated, and fever lessened : still some cough, and complains of great debility.

On the 23d, I gave him some of the tr. cinchon. compos. under the hopes that the inflammatory action was pretty well subdued, and that the present symptoms might result from relaxation and want of energy in the absorbent system of the organs of respiration. I was not apprehensive, at this time, that any thing serious would ensue, supposing it to be a mild pneumonia or catarrhal fever, which was prevalent, and therefore was not so decided in my practice as I otherwise would have been ; and what contributed to my relaxation, was his extreme aversion to medicine, and a persuasion that it was nothing more than a bad cold.

He complained now little of any thing but the cough, which was very troublesome, particularly at night. He called on the 29th, and wished some paregoric to relieve his cough ; for he thought if that was easier he would be well. I replied, no doubt the great object was to subdue the cough, but I was apprehensive it would require some other treatment. However, I indulged him with the paregoric.

May 1, Finding himself no better, but that the cough increased in violence, that there was a great stricture in his breast, dispnœa, chills, and fevers, which exacerbated in the evening, and night sweats, with great debility, I stated to him the indispensable necessity of pursuing some other plan for the recovery of his health, as he was threatened with an an alarming disease. He consented to take medicine. I prescribed a dose of neutral salts, and applied a large vesicatory over his breast.

2. The medicine operated well, and the blister rose finely. Felt something easier in his breast. Gave him a pectoral mixture of ipecac. g. arab. Tr. opii and pure water, to be repeated in the quantity of a spoonful frequently through the day.

3. The symptoms not alleviated. The stricture and difficulty of breathing are great—cough very severe—fever continues. Gave him powders, composed of nitre and ipecac. to be used in such a manner as would excite nausea, and produce a diaphoresis.

4. The powders sickened him so excessively, he discontinued them, after using two or three. Not much change.

5. There is evidently an affection of his lungs. He has chills, fevers, and night sweats ; his cough severe, and expectoration very copious, perhaps to the amount of a pint during the night. Stricture of the breast great, loss of breath upon the least exercise, want of appetite, flushing of the cheeks, &c. To-day I found it would be indispensably necessary to adopt some more decisive plan, in order to check the rapid progress this fatal foe to human happiness was making, in destroying the all-important organs of respiration. I was well aware of the perplexity in which every feeling mind must be involved, when attempting to digest a plan of attack, which would insure success against an enemy the most insidious and unmanageable that afflicts humanity. I was well assured of the futility of recommending a mercurial course, in consequence of the impressions the patient had, both as it respected the nature of his disease, and the danger and fatal effects of mercury. In reviewing the various remedies which empiricism or false reasoning have recorded as infallible, I could not refrain from the

gloomy reflection, that as yet all the boasted powers of the healing art had been rendered impotent and lifeless, by the all-destroying canker of phthisis pulmonalis. What should I do? I resolved on trying the combined effects of the acetate of lead and tr. digitalis. I must confess that I was led to this decision, not only from the general effects ascribed to these medicines on the human system, particularly in this disease, but from the manner in which Dr. Reese speaks of them, and also Dr. Little, as contributing much to subdue the fever.—I applied to-day a pitch plaster over his breast, which was yet tender from the blister, and also one between the shoulders. I likewise gave him the acetate of lead, in pills, to the extent of 2 grs. three times a-day, and in the intervals the tr. digital. to the amount of 10 drops. His pulse, for a considerable time past, has been very frequent, and rather small, though often too tense; but I could not induce him to lose blood.—His pulse generally at this time counted one hundred.

6. No perceptible change. Continue the medicines.

7. The cough somewhat alleviated, pulse not so frequent, and more soft. Continue the medicines, increasing the tr. a drop every day.

8. A change somewhat favourable: pulse better, counts from 90 to 100.

9. The symptoms are lessening perceptibly in severity.—The cough not so troublesome—expectoration not so copious, and the pulse less frequent. Continue.

10. Continues better. Expectoration diminished very much—fever very inconsiderable—pulse nearly natural—rests pretty

well at night—appetite returning. Continue. Increase the sac. saturn. to 3 grs. three times a-day.

11. Continues better.

12. Continues better. No fever of any consequence—no chills, cough abating much—pulse as slow as 70, and natural.

17. Relieved of almost every symptom of disease—appetite good—bowels regular—no fevers or sweats—no cough, unless he walks smartly, or uses much exercise. Continue.

19. Not so well. He exposed himself too much in bad weather. Days cold, and temperature variable. I cannot induce him to wear flannel. Cough much more severe, and stricture considerable in the breast. I took some blood from him to-day—had an inflammatory coat.

20. Felt immediate relief by blood-letting yesterday. Still too much action in the arterial system. Cough troublesome, and appetite not good. Took about 10 oz. more blood.

22. Better. The stricture is nearly removed—the cough not so troublesome—pulse pretty natural. Takes the medicine as usual.

23. Continues better. Walks through town.

31. Continues free of every symptom of disease, except a slight cough occasionally, which is produced by too much exercise. Takes moderate exercise in the garden. Appetite good, and strength increasing fast. Permitted him to discontinue the digitalis. Continue the sac. saturn. a short time longer, with the addition of the nitric acid.

This patient has remained free of every pulmonary symptom ever since the last date, and is now in pretty good health. But it is to be lamented, that we find so frequently verified the affecting truth, that there is no unmixed good allotted man in this terrestrial abode. This patient owed a scene of considerable suffering to the very cause which contributed to snatch him from the abodes of rottenness. In the course of 6 or 7 days after the last-mentioned date, he was attacked with a severe pain in the epigastric region, which extended from side to side. From the nature and symptoms of this affection, I had no hesitation in ascribing it to the action of the lead. I found it obstinate and difficult to relieve; but I finally succeeded in destroying every remnant of the disease. But I trust I am warranted in saying, that this effect of the lead may be avoided by administering it more sparingly, and yet answer the purpose of removing the pulmonary disease. For it may be observed, that I administered it very liberally, and that I increased the quantity after I was assured the disease was yielding, from a desire that the impression should be permanent, and that every opportunity might be given to prevent any future indisposition*.

I have, however, endeavoured faithfully to delineate the case, in every view which I conceived might be of future advantage to the medical world; and in whatever light it may be viewed by them, I shall flatter myself that I have done my duty.

SAMUEL AGNEW.

Harrisburg.

* A query might be made, how far the nitric acid added to the subsequent unpleasant symptoms, for, by this addition, the patient probably was taking a nitrate of lead, and not an acetate. What the effects of nitrate of lead are upon the system, I know not. EDITOR.

Account of Hanah's Sulphur Spring. In a letter from SAMUEL AGNEW, M. D. *to the* EDITOR.

Harrisburg, 13*th September*, 1809.

SIR,

I INCLOSE you a brief and imperfect account of a visit I made to a mineral in the adjoining county of Cumberland. My object was to endeavour to ascertain, by a few experiments, some of the more evident properties of the water. But being very imperfectly acquainted with chemical experiments, and having but a few hours to stay, I obtained but little satisfaction. I have given a statement of the appearances of the water, and the depositions about the spring, with a few experiments, from which, if you can draw any satisfactory opinion, even of the more prominent qualities of the water, I will thank you much for your communication of them. I have little doubt of its possessing valuable properties, and am desirous that the proprietor, who is in rather embarrassed circumstances, should have it in his power to benefit himself as well as serve the public.

I am, Sir,

Your humble servant,

SAMUEL AGNEW.

Dr. J. R. Coxe.

I visited this water, in company with S. Laird, Esq. and two other gentlemen, on the 20th and 21st May, 1809. It is situated in a narrow valley or recess, formed by the two ends of the North mountain, which appears to be interrupted here, projecting a little past each other. It is about 17 or 18 miles W. something N. of Carlisle, and perhaps 6 miles N. something

E. of Shippensburgh, in the county of Cumberland, Pennsylvania. The place is wild and romantic, well calculated to delight the eye of him who is fond of the grand and awful productions of nature. It is situated on a dry bank, several rods above a small stream, which runs down the valley, and has a S. or S. E. exposure. It is surrounded by timber, and is almost entirely in a state of nature. The soil appears to be composed of clay and a large quantity of quartzose gravel. There are very few rocks in the neighbourhood, and these small, on the surface of the ground. They appear to be formed of quartz and granite, perhaps in a bed of schistus. I was informed by Mr. Hanah, that the rock from which the water issues, is a slate of a dark colour. I could not have access to this, as the spring is enclosed in a hollow gum set in the ground between 2 and 3 feet in depth. A stream issues from an auger hole of about one inch diameter, and is conveyed off by a small trough formed by the excavation of a small sapling.—On my approach to the spring, I discovered a sulphurous smell, better than a rod distant. It is necessary to observe here, that this was an unfavourable time of the season for examining the water, as the spring has been unusually cold, and of late several rains. Mr. Hanah informed us, however, that the weather made very little impression on the quantity of water, but that in the warm season it emitted something of a stronger smell of sulphur.—The water is remarkably clear and transparent, and of an agreeable temperature. In looking into the spring, confined in the gum, the eye is delighted with a variegated tissue of colours, but that of a purple strongly predominates, making the sides of the gum appear like the finest purple silk velvet. This is confined to about two-thirds of the distance from the top to the bottom. The upper part of the gum has a large quantity of a white substance adhering to its sides, and floating with its fibres in the water, resembling small flakes of wool or cotton,

except that, when you catch it with your fingers, it appears to dissolve, or be dissipated, like the pulp of fine white paper, which has been a long time macerated in water. This substance also adheres in large quantities to the sides and bottom of the trough which conveys the water from the spring. Mr. H. informs, that, if removed, it will form in the course of 30 or 36 hours again. By a close inspection of the surface of the spring, it may be observed to be under constant agitation, not general over the surface at the same time, but at different points, forming a kind of undulation, similar to what takes place when a small insect or particle of dirt falls on the surface of a placid stream. This I was willing to ascribe to the escape of carbonic gas. The water is certainly light, a person being able to drink 4 or 5 pints in the course of twenty minutes. Those who have used the water for a short time, can drink it in much larger quantities. It smells and tastes very evidently of sulphur, but not so strong as some other waters I have seen.

EXPERIMENTS.

1. By adding a small quantity of sulphuric acid to some water in a wine glass, a great intestine motion and effervescence ensued, and a white precipitate.—This, I think, detects the existence of *carbonic acid gas*.

2. By adding a solution of carbonate of soda to about an equal quantity of the water, and holding a glass stopper, moistened with muriatic acid, over it, white fumes or vapour was evidently seen.—This is said to detect the existence of an *ammon. salt*.

3. By adding a solution of the acetate of lead, a white cloudy

precipitate or flocculi immediately appears, and the whole after a short time becomes turbid.

4. By adding some nitric acid, it appeared to lessen the sulphurous smell.

5. By writing with a solution of the acetate of lead on white paper, and holding it over some of the water heated, the letters became of a light copper colour, in the course of 15 or 20 minutes.

A number of other experiments were made, but I was unable to draw any conclusion from them. The water is represented to have had a very happy effect on many diseased persons. It had a very powerful and salutary effect on Mr. Laird, who is generally in a very costive state, and of a bilious habit. Its effects were not so evident on the rest. It is a diuretic.

History of the Typhus Petechialis, or the Malignant Petechial or Spotted Fever; as it appeared in Goshen, Connecticut, during the Winter of 1807–8; *with such Remarks as may tend to elucidate its Nature, and to establish the best Method of Cure. By* Elisha North.

Goshen, February 12th, 1809.

Dear Sir,

AGREEABLY to your request, I will now communicate to you the result of the knowledge I have acquired respecting the epidemic which prevailed here last winter.

This epidemic first appeared in the town of Winchester (about eight miles distant), in the spring of 1807.

According to the best information I have been able to obtain, one-third of those who had it died: a great mortality.

The physicians, although some of them were skilful in other diseases, knew neither the nature of this nor the method of cure, until, at length, one was cured by sweating.

Sweating was excited by exhibiting a warm tea made of the twigs of that species of fir called hemlock, and applying blocks of the same boiled in water, and wrapped in cloths, and laid into bed near the patient. This practice was first used by a Mrs. Hurlbert upon her own child, without the advice of a physician, or without the knowledge of the disease. Previous to this all had died. After the cure of Mrs. Hurlbert's child, she was active in introducing her method of practice, and some of the physicians readily adopted it. Afterwards fewer died.

From the inquiries I had made with respect to this disease at Winchester (for I had not had an opportunity of seeing it), I was unable to satisfy myself of its nature, or the best method of cure.

At length it appeared in this town. The three first patients died with it, and were buried in one grave. I saw neither of these. One of the first patients I saw was a young woman eighteen years old. She died within twenty-four hours. She died in what I shall call, in the progress of this inquiry, the *sinking state of fever.* Immediately upon the death of this

patient, I adopted the stimulating practice; with what success you will see in the sequel. At this time, patients beginning to thicken upon me, I happened to see a skilful physician, who had seen a few sick with this disease. I informed him of the plan I was pursuing. He cautioned me thus: "Beware of your stimulants, for the disease is an inflammation of the brain." I told him my remedies *appeared* to afford relief, and I should not alter my plan until I had further evidence. Soon after this I received your letter, which tended to confirm me in my plan, and for which I return you my most cordial thanks. Thus much by way of preface.

The symptoms are the following:

Pain of the head, more commonly the back side; slight chills; furred tongue; great prostration of strength, early in the disease; loss of appetite, although less, I have thought, than in other fevers, especially in the lighter cases; sometimes vomiting and purging; distress about the precordia; pains of the limbs, frequently; sometimes a slight cough; pulse generally weak and quick, sometimes full, but never hard, as I have judged. In some cases the pulse is so little removed from health in quickness, although weak, that I have doubted whether such could fairly be said to have fever in the common acceptation of the term; these had, apparently, little or no febrile heat. Others had great heat, and apparently a high fever. Some had a sweating in the progress of the disease.

If I have not deceived myself, almost all had a kind of œdematous feel of the skin, especially about the hands and wrists. Dejection of countenance.

Upon inquiry, almost all would tell you that they had, in the commencement of the complaint, a slight sore-throat, although few would mention it of their own accord. In a few, but very few, however, I have been able to discover aphthæ on the tonsils. In general, they would tell me the soreness was a little lower down in the throat.

In the bad cases, the most distressing symptoms were pain of the head, and universal distress and agony, which would cause children to draw back their heads, and toss and throw about their limbs: these had a constant sighing and quickness of breathing. Some had delirium. It was sometimes low; in others it was a violent mania. Some were comatous.

With regard to the spots or petechiæ, from which the disease appears to take its name, they are by no means a constant or frequent symptom. When they do appear, they are of various sorts. Some have spots in the true skin (they seldom rise up into pimples like other eruptions) resembling flea-bites. One patient was covered all over with such spots, for a number of days. But more commonly you will find only a few scattered on different parts of the body. They are of different grades of colour, from a red to a dark colour. Some resemble a bruise; others appear as though the patient had been struck with a whip. One patient had a little spot upon her arm; she thought it occasioned by the bite of a spider: from this spot arose an erysipelatous inflammation, which extended over her whole fore-arm, of a dark-red colour, which appeared on the point of a mortification. This symptom was removed within twenty-four hours, by the use of bark and wine.

Some have been attended with slight hemorrhages from the nose; others with symptoms resembling hysteria.

The length or duration of the disease has been very various, extending from one week to five or six. Indeed, many have been in a convalescent state much longer.

I have not often observed any thing like a regular crisis, so often observed in other fevers. In a few of the last cases, however, which occurred (in these the fever followed the measles), the fever attacked the patient like the spotted fever, but soon assumed the form of a common typhus, and terminated by a regular crisis. When these cases occurred, the sickly season had extended into the beginning of summer.

In the close of the same summer, the common typhus fever broke out among us, and has been very rife. This serves to corroborate Sydenham's opinion of the varying nature of epidemical diseases.

Although the typhus fever has been evidently very contagious, yet the spotted fever has appeared not to be communicated by contagion.

Death happens in the spotted fever many times suddenly and unexpectedly, and when there is apparently little danger, at least to common observers.

I shall omit describing the sinking state of this fever, until I come to treat of its cure.

Children and young persons are the most liable to this disease, although I have heard of one woman, who was sixty years old, who died with it. Those of a sanguine constitution and feeble habit, are more liable to it than others.

I have been of opinion, that such causes as had a tendency to produce a debilitated and scorbutic habit of body, must be considered as the predisposing. Of these may be reckoned a moist atmosphere, and less nutritious food than usual. Two seasons preceding this epidemic, have been uncommonly wet: so much so that we have had very little corn, less garden vegetables, and bad grain.

An observing miller has told me, that he never ground so much bad rye in his life: this is the common bread-corn of the inhabitants.

The most frequent immediate exciting cause has been obstructed perspiration by cold.

METHOD OF CURE.

The indications of cure appear to be:

1. To evacuate the first passages.
2. To restore the obstructed perspiration.
3. To invigorate the solids.
4. To correct the scorbutic tendency of the humours.
5. To obviate accidental death.

If I am called in the commencement of the complaint, and discover no symptoms immediately alarming, I first give an emetic, commonly of ipecacuanha, then a cathartic of senna,

rheubarb, or some other mild purge. This I do to prepare the stomach and bowels the better to receive other medicines. While this is doing, I apply a blister on the nape of the neck: this will, in ordinary cases, remove the head-ache. I also direct elix. paregoric, or tinct. opii, in sufficient doses to ease pain and remove distress, to be taken two or three times a day, as long as the patient chooses; also gum camphor reduced to powder, with a little spirit and white sugar, four or five grains, once in two or three hours. Elix. vitriol, and native acids, if they are to be had, are also used. Besides these, I direct wine, or spirit of wine, diluted; wine I have generally preferred: of this take from half a pint to a pint daily.

To the above, I frequently add a tea of Virginia snake-root and Peruvian bark. Of this let the patient take, if he be an adult, two ounces, once in three or four hours. Keep the bowels in order. I see that the patient has a supply of fresh air. Keep his feet warm, either with sinapisms or bottles of warm water. Apply blisters occasionally. Order for diet such as the patient's stomach will best bear.

I sometimes, to vary my medicine, order tinct. cort. Huxh. tinct. castor. com.—ess. menth. pip.—spt. lavend. com. &c.

For common drink I direct warm teas: such as sage, pennyroyal, hemlock, and rob of elder, diluted.

Such a kind of course, in ordinary cases, varying occasionally according to circumstances (keeping in mind my general indications), I have found to be sufficient, in time, to effect a cure.

I will now attend to my 5th indication, which is to obviate accidental death. Here I shall premise a few things, to explain what I mean by accidental death.

In this fever, in the malignant intermittent fever, in the yellow fever, in the angina maligna, in the plague, and other fevers, patients sometimes die suddenly and unexpectedly, at or near their commencement; sometimes in their progress. Now, the causes of death, in such cases, may be supposed different from those in which death is produced by a long and violent course of fever; or, if the proximate cause is the same, it may reasonably be supposed, that the chance of obviating the proximate cause of death, by remedies, must be greater in the commencement of fever, than after fever has continued so long as to injure or destroy the organization of the body. That we are very much in the dark respecting the nature of fever, I readily acknowledge. I wish, therefore, to theorize no further than is necessary to convey to you a practical idea respecting the cure of fever. I am little solicitous whether the theory be true in fact, provided I succeed in communicating my thoughts so as to be understood so far as respects the method of cure.

Suppose a man ninety years old to die a natural death; would you not say that the excitability or irritability of his system had been so worn out by the reiterated application of stimuli, which had been applied, during a long life, that it would be in vain to attempt to resuscitate him by the application of fresh stimuli?

Suppose another person, say a child, to have his excitability or irritability suddenly suspended by drowning, so as to be ap-

parently dead; would you not attempt, in this case, to rouse the irritability by the application of fresh stimuli, and thereby restore life?

Suppose a person to have a fever which shall continue to progress so long as to destroy the organization of the body, and to wear out the excitability of the system, and cause death: in this case it would be in vain to resuscitate him.

Suppose another suddenly attacked with fever, and, by the violence of the pain, or excessive fatigue, or other causes, the excitability or irritability is suddenly suspended, and apparent death produced; would there not be some chance, in this case, that the application of stimuli might resuscitate him? Dr. John Rush has given us one case in which this was actually done.

The first may be called the natural death of fever, the second accidental death.

That what is here called accidental death produced by fever, may be frequently prevented by the timely and thorough application of remedies, is a fact of which I have not the least doubt.

Having theorized thus much, I am prepared to proceed with my curative indication in the sinking state of fever.

If I am called to a patient attacked with spotted fever, and find him attended with violent pain of the head, drawing back of the head, tossing about the limbs, sighing, distress for breath, coldness of the extreme parts, sickness at stomach, great distress and agony, or comatous, with an involuntary

running of the tears, lividness of the lips, or with delirium or mania, with a low and feeble pulse, or, perhaps, none at all, I conclude that there is no time to be lost: that he is in danger of dying in a few hours, either by syncope or pulmonary convulsions or otherwise.

I immediately set myself to work upon the patient with stimulants, externally and internally. I put the patient to bed, after having first bathed his feet in warm water, and then apply a blister to the nape of his neck; give tinct. opii, hot brandy-sling, heated wine, warm teas. I also apply hot blocks or brands from the fire, quenched in water, and wrapped in cloths, near the patient, in his bed. Also bottles of warm water, or similar blocks, to his feet. In some of these cases I have given ol. peppermint, essence peppermint, tinct. castor. com.—camphor, &c. taking care not to crowd the stomach so as to excite vomiting. Generally, as soon as the patient grows warm, he is relieved; not always, however, until he sweats. I have known one case in which the patient sweated an hour without relief: in that case I gave a second dose of opium, and the patient soon became easy. In these patients (after this process had relieved them) the fever appears as in other cases, and is to be treated accordingly.

I have always been careful to direct the nurses, if any symptoms of sinking occurred at any stage of the fever, to give some stimulants at such times, as a little wine, &c. By such practice I have not lost a single patient since the one already mentioned. I have treated perhaps one hundred.

I do not suppose the cure depends upon the mere flowing of the sweat, but upon the effect of the stimuli used for exciting

the sweat. Still I consider the sweating as useful, it being a kind of medical thermometer to enable us to know when the degree of stimuli has been carried sufficiently far in the bad cases.

In consequence of these opinions, I have not directed the sweating process in more than one-sixth part of my patients, and perhaps many of these might have done as well without. But, as I have observed no injury to arise from the practice, and knowing the liability of sudden death, I have considered myself as erring (if I have erred at all) on the safe side, when I directed the practice, provided the sweat was not allowed to flow too long.

I shall conclude with a few remarks upon the practice of others.

In Winchester, bleeding was tried. It was thought it hastened the death of some. In other places it has been tried, and with like success. And yet I find Sydenham bled once in his febris nova, which was a similar fever with this. He, however, cautions against repeated bleeding. His practice was a mixed practice: for he used blisters between the shoulders; also anodynes and acids; also bark, where he discovered aphthæ. Sweating he disapproves of. He sweated his patients twenty-four hours. This might do harm, by dissipating the caloric of the system, and weakening the patient. That he sweated his patients in this manner appears from another part of his book, viz.: where he describes the utility of sweating in the epidemic fever, which prevailed the year after the plague.

The physicians who adopted Mrs. Hurlbert's mode of sweating, applied it to every patient, not knowing but there might be some specific virtue in the hemlock tea. This is so

far from being fact, that I do not think it so good as many other teas. It disagrees with some stomachs.

I am decidedly of opinion, this disease requires stimulating remedies: but by this I do not mean that in every case patients must take brandy by quarts, or wine by gallons; for I believe with you, sir, that "it requires as much judgment in using stimulants as it does in reducing the system."

I am, sir, with much respect,

Your obedient and humble servant,

ELISHA NORTH.

Dr. Elijah Munson, Newhaven.

Cases of Bilious Fever produced by Vegetable Miasmata. By Dr. JOHN A. CASEY.

Augusta, Georgia, 1st October, 1809.

DEAR SIR,

A REMARKABLE instance of the influence of putrescent vegetable matter producing a highly inflammatory bilious fever, occurred in this neighbourhood a few weeks ago. A more convincing case I do not recollect to have seen.

It is the practice of our citizens, at least of the wealthy part, to retire to country situations during the summer and fall months. Some have pleasant retreats on the Sand Hills, situated three miles west of the town, amid piney woods, and removed from mills or stagnant waters of any kind. Others retire to plantations in more fertile parts of Columbia, the county adjoining. Mrs. —— had returned early in the summer, to this latter place, while the negroes were at work in clearing land: during the summer, one hundred and fifty acres were cleared. The miasm from this immense quantity of vegetable

decaying matter was wafted by every breeze to the dwelling, which was unprotected from its influence. Twelve in family made this place their summer abode. Towards the latter end of August, they began to sicken, and before the middle of September, every individual was sick. One died, and the cases of all were severe, highly dangerous, and slow in their recovery. Venesection and copious evacuations by the bowels were necessary. The discharges were black, such as take place in yellow fever, and the skin and livid appearances were such exactly as I have observed in yellow fever. Of a few visitors to the above-mentioned place, two were taken ill. The symptoms in all were alike, i. e. a pain in the head, back, and bones, very furred tongue, inflamed eyes ; high fever introduced by a considerable fit, and immediately after assuming the remittent form, which, in the convalescent period, was exchanged for a quotidian intermittent, commonly called the ague and fever. Even with generous nursing, a free use of bark and wine, the restoration was very tedious. Of the two visitors who were ill, one died. The symptoms which occurred during the illness of those that died, were the same, excessive thirst, a burning sensation at the stomach, general coldness; several days before dissolution, the extremities felt more like damp marble in a bleak winter's day; after death a yellowness of the skin, with purple streaks and spots, and a speedy disposition to putrefaction. In one case, it commenced almost with the termination of life.

This tragic scene befel two of our most worthy and respectable families. They were of course very sedulously attended to, and though they were watched with the greatest care, one for fifteen, the other for five days, yet not one of the attendants became indisposed. I had forgotten to remark, the family were brought into town, early in their attack, for the more readily procuring medical aid. Respectfully,

Dr. J. R. Coxe. JOHN A. CASEY.

MEDICAL AND PHILOSOPHICAL
REGISTER.

VOL. VI.....NO. I.

FOREIGN AND DOMESTIC.

Observations on Quackery, &c. continued from the Medical Observer.

NO. III.

" *By the King's Patent,*

" GODBOLD'S VEGETABLE BALSAM,

" *An infallible remedy for consumptions, asthmas, and coughs.*"

MR. GODBOLD prefaces his advertisements with the names of 1 marquis, 3 lords, 5 ladies of distinction, 4 baronets, 3 generals, and 1 colonel, who (he observes)

" Having, for more than twenty years, experienced the efficacy of *Godbold's Patent Vegetable Balsam*, for the cure of coughs, colds, asthmas, and consumptions, in their own fami-

lies, as well as witnessed it in others, particularly recommends it to be kept by all persons, to have recourse to upon the first attack of either of the above complaints, in which case it never fails of success, and in hundreds of instances it has performed cures after patients have been given over by *their physicians*, by which means, a *great number of the faculty* conjoins in the recommendation, having admitted it as the first medicine in the world for the above disorders.

" Numerous certificates from others of the nobility and families of distinction, as well as from the above, to whom references may be made, may be seen at the proprietor's, Bloomsbury square, which are *proofs* of its utility beyond the power of envy to dispute."

" N. B. To prevent *fraud*, their name is written upon every label, and printed at full length upon each stamp that covers the cork of each bottle."

On examining this nostrum, we do not discover any property that can possibly entitle it to the appellation of a balsam, but the propriety of the term vegetable, we cannot dispute, as vinegar, sugar, and honey are vegetable productions; we can, however, positively deny that it possesses the balsamic property of vegetables, and our examination, as well as the trials we have known to be made with it, do not justify our attributing to it any virtues superior to the *simple oxymel* of the shops, although sold at the very exorbitant rate of 18*s*. per pint, for which a regular chymist would be ashamed to ask as many pence. Why a patent was granted to the proprietor for this medicine, we are at a loss to conjecture.—It is a sanction which has a great influence with the ignorant, and which we consider it by no means entitled to.

The proprietors in their public advertisements bring forward the names of a few noblemen* in testimony of its efficacy. The attestation of people unacquainted with medicine, and so ignorant of diseases as not to know a common catarrh from real consumption of the lungs, can have but little weight with the thinking part of mankind. One would, however, have supposed, that such characters would have had a greater regard for their own reputation, and that of their family, to have suffered their names to have been recorded in support of empirical practices.

Consumption of the lungs is not a *primary* affection, but the consequence of some previous disease of the lungs, arising from a variety of causes; it has also different stages, which require as opposite a treatment as any two diseases we are acquainted with. The medicine that is proper in one stage, must necessarily be injurious in another, and which again must be varied according to the nature of the primary complaint and other peculiarity of constitution. The celebrated Sauvages has enumerated no less than twenty species of consumption of the lungs, all arising from very different causes. Is it, therefore, possible, that one medicine can be applicable to all? It requires no medical skill or pathological knowledge to answer this question.

It has been stated in favour of this nostrum, that if it do no good, it is incapable of doing harm. Every person acquainted

* On the most unquestionable authority we can state, that the finest turkey Norfolk can produce, is sent to one of these right honourable testifiers, as well as several similar presents in the course of the year; we apprehend for the use of his lordship's name. It is, therefore, a fair presumption, that the rest of the respectable list have their share of rewards in proportion to their ranks in life!!!

with the disease that takes place in the lungs, must know that a medicine which has no power of suspending its progress must be productive of mischief, inasmuch as the patient, by trusting to it, loses an opportunity never to be regained. Such is the nature of this disease, and the delicate structure of the lungs, as well as their importance in the animal œconomy, that instead of palliative, harmless, or doubtful remedies being employed, it requires great experience to discover the real state of the disease, and often a bold and decisive practice to save the life of the patient: the delay of a day is a serious loss to such patients. Dr. John Reid, a physician of great erudition and professional judgment, in a treatise on consumption of the lungs, just published, makes the following very pertinent observation: "Many consumptive affections have been originally implanted in the nursery, &c. and made to expand by *infallible remedies* for coughs, colds, and consumptions: that hardy empiricism, however, which for individual profit occasions permanent and irreparable injury to the health, and consequent happiness of either unconscious or unsuspecting individuals, deserves in an equal degree the severity of reproach, nor can the plea of ignorance of consequences justly claim any thing further than a slight mitigation of the austerity of censure. Consumption is a vast pit-fall situated in the high road of life, empiricism is the treacherous hand, which under false pretences conducts to its margin, and precipitates the fatal descent. When the English nation shall be firmly convinced that *sweeteners of the blood*, *antiscorbutics*, remedies for colds, and, according to the observation of the astonished Chinese philosopher, *for every disease to which the human frame is subject*, are either altogether inert, or highly injurious by their indiscriminate administration, &c. diseases in general will be of less frequent occurrence, and the list of consumptive affections will undergo a very considerable diminution."

That able physician, Dr. Fothergill, in the London Medical Commentaries states, that it was impossible to lay down a general rule even for the treatment of a particular species or stage of pulmonary consumption, as he had often known a medicine that proved beneficial in one instance, considerably to aggravate the symptom in another similar case, through a peculiar irritability of the system; even the saline mixture he had found to increase all the febrile symptoms. Dr. Reece, in a dissertation on pulmonary consumption, enumerates several species of the disease, for which he recommends very different treatment. This author states, that from the number of consumptive patients that have applied to him for relief, on an average, nine out of ten were taking quack medicines; this author's observations on the danger of advertised nostrums we have taken the liberty of quoting in our notice of the balsam of liquorice.

We are surprised that Mr. Godbold has not mentioned the names of those physicians who declared the patients that were cured by his nostrum to have been irrecoverable, or that the cases were *true phthisis pulmonalis*, and that he should omit the names of the members of the medical faculty that have, in consequence of those cures, adopted its use in their practice. We are persuaded that if this medicine really manifested any *specific effects* in pulmonary consumption, there is no medical man in this kingdom but what would very readily and gladly come forward to attest the same, which would more powerfully have recommended it to public notice, and be far more honourable to the proprietor, than the testimonies of people who are both ignorant of diseases and medicine.

If those gentlemen really derived the advantages from the use of this nostrum, which the public advertisement intimates

(for we never heard of their being consumptive), their conduct should have been very different from what it has been. They should first have ascertained the *real merits* of the medicine, and if experience had proved it to possess antiphthisical properties, or that it had in one instance cured a patient, declared by a physician to be really afflicted with consumption of the lungs, they should have advised the proprietor to have applied for parliamentary reward, instead of suffering their names to appear in favour of empirical practices. If Mr. Godbold, notwithstanding his education and situation in life, had discovered a remedy for a disease that at least destroys 30,000 of his majesty's subjects annually, he would have been entitled to a very handsome remuneration from his country, and which no doubt would have been granted by parliament. The medicine of course would have been sanctioned by the faculty, and the proprietor might also have had the privilege of making it secure to him by virtue of a patent, which would have tended more to his emolument, and redounded more to his honour. We have known this medicine tried in many very fair cases of pulmonary consumption, without evincing any salutary effect whatever, and we are inclined to believe that the proprietor would be very sorry to risk its reputation by putting it to the test of experience.

As a proof of its inefficacy, and the great IMPROPRIETY of noblemen permitting their names to appear in testimony of the supposed virtues of any medicine, we insert the following case, communicated to us by a medical man of respectability.

TO THE EDITORS OF THE MEDICAL OBSERVER.

Gentlemen,

If the following account of the effects of an advertised *specific* for pulmonary consumptions, with the letters from the proprietors, accord with the nature of the publication announced on the cover of the Physical and Medical Journal, you have my permission to publish them.

In the month of January, 1805, I was requested to see a gentleman at Exmouth, in Devonshire, afflicted with that disease of the lungs, termed pulmonary consumption. In the history of his complaint he gave me to understand, that he had been for some time under the care of a very celebrated physician, from whose advice he derived considerable benefit, and in conformity to which, he had taken up his residence at that place for the winter season. Here, it appears he was prevailed on *by a friend*, to try "Godbold's vegetable balsam," which he agreed to, in consequence of the public recommendation of that medicine by a nobleman, of whom he had some knowledge, which one would have supposed, with a gentleman of his sense and education, would have had no influence. His lady accordingly sent his case to Mr. Godbold, who returned the following answer:

" *Madam,*

" We received your Letter and have attended to the statement of your Husbands case, we are sorry he did not apply to our Balsam sooner, which is the misfortune of people under the like affliction *to* often, however we have cured *hundreds* as ill as he seems to be, and *some whose bones have perforated the skin*, under these circumstances we wish you not to despair, for if *any Medicine* in *the world* can restore him, *the Vegetable*

Balsam will which we could put up a little more applicable to his disorder than what you purchase of our *venders*,* should it meet your approbation. We totally *disaprove* of Laudanum— but the drying up of his Issue we think right his fever is occasioned by his weakness and not from the nourishment of his beverage—we wish you to keep his body open and NOT TO SPARE THE BALSAM ON ANY ACCOUNT for it will not effecct his *bowels*, but act as a strong Febrifuge and is both a *searcher* and *healer* of the *lungs*, let him take the balsam *every time* the cough is troublesome, *over* and *above the Quantity* prescribed in the printed bill of directions let *him live well* eat any thing he fancies (salt meat and high seasoned dishes excepted) take malt liquor with a toast in it at his *diner*, and a glass *or* red port afterwards use gentle exercise such as his strength and the weather will permit, but not to fatigue, and be very careful not to *get* cold, the Electuary will give one gentle motion and in case of a relax such would be occasioned from the disorder and not the opening medicine then the powders is used.

We are,
Madam,
Your most obedient
Humble servants,
(Signed) N. & S. GODBOLD.

Bloomsbury Square, Nov. 21, 1804."

N. B. " If you wish any medicine to be put up from our house with the attendants *will be two pounds* but we may say 99 out of 100 Persons in the first stage of these complaints through *God's Blessing* a cure is *performed.*"

* It may be necessary to observe, that retailers of quack medicines are allowed from 30 to 40 per cent. which, of course, the proprietors save by selling the articles themselves.

In consequence of this advice, the gentleman discontinued the use of the medicines prescribed by an able physician, healed the issue in his side, and took to animal food and wine, which he had been strictly forbid to do by the physician who had attended him. This *healing* and *searching* balsam was taken according to the directions, which, so far from affording him the promised relief, disagreed with the stomach, and was rejected, as sour as the strongest vinegar. The hectic symptoms were also considerably aggravated, which, however, I attribute more to the alteration in the diet, the use of wine, and drying up of the issue, than to the medicine.

Mr. Godbold was immediately apprized with the disagreeable effects of the medicine, to which he returned the following answer:

" *Madam,*

" In answer to your favor received this morning we assure you there is no *accidity* in our medicine whatever nor will it create any, nor has your husband any thing to fear by a perseverance in taking it—we recommend his taking the Electuary as it is highly necessary his body should be kept from costiveness he cannot take any thing better for the *bile upon his stomach* than Magnessia with a little *Rubarb* mixed with it—and now and then by way of change let him take a teaspoonful*l* of Epsom salts in a little mint water about 11 o'Clock in the forenoon—a child *sucking at breast* may take our medicine *whenever you think proper we will compound a bottle of medicine for him.*

We are Madam,
Your most obedient servants,
(Signed) N. & S. Godbold.

Bloomsbury Square, Nov. 26, 1804."

Notwithstanding this medicine is made with vinegar, Mr. G. does not scruple to assert in the most positive terms, that there is no acidity in it *whatever ! ! !* and although he declares it to be a pure *vegetable* balsam, yet it will not create an acidity ! ! !—So far from this being the case, the very article with which it is sweetened is capable of forming one of the strongest vegetable acids, and which there is no article in the medicine capable of counteracting.

The gentleman could not be persuaded against his reason to persist in the use of the *balsam* he then had, and accordingly he wrote to Mr. Godbold to send him a *fresh* bottle, with such alterations he thought proper to make, to which he received the following reply :

" *Madam*,

" We received your favor and 2*l.* note and have sent the medicine accordingly as you directed which we hope and trust with the *blessing of God* will have the desired effect—we have sent a double quantity of the Electuary and whenever he has that heaviness upon the stomach let him have recourse to it which will carry it off—and *spare not the balsam* by *any means*. You may depend upon its *creating no aceddity* whatever in other respects follow the printed directions *excepting when the cough is troublesome* then *take it extra*—live well and be careful not to take cold.

We are

Madam,

Your most obedient humble servants,

N. & S. Godbold.

Bloomsbury Square, Dec. 1, 1804."

This fresh medicine, however, was attended with no better effects, and finding his symptoms to increase rapidly (particularly the difficulty of breathing, thirst, and hectic heats), he resolved on discontinuing its use entirely, and re-applying to the regular profession. When I saw him, which was in a few days after, his pulse, which was about 90 at the time the issue was dried, was now increased to 120, and sometimes more, his breath very short and laborious, with frequent recurrence of rigors, succeeded by heat and perspiration. In order to give him every chance of recovery, I advised an issue in the side most affected, forbid the use of wine and animal food, and requested he would live as much as possible on ass's milk and vegetable jellies. I also ordered him a little medicine to quiet the system and facilitate expectoration, which relieved the most distressing symptoms, and rendered his life much more tolerable; but unfortunately the lungs had sustained so much injury that they were no longer capable of effecting that change in the blood which is necessary for the support of life. Whether this organic mischief might have been prevented by keeping the issue open, by quieting medicines, and a judicious regimen, I cannot take upon me to determine. I think, however, every medical man must agree with me in opinion, that he would, in that case, have stood a better chance of recovery.

To give you an idea of the absurd and dangerous doctrines of quacks, I annex the following case.

In May, 1804, a young lady, of very delicate habit, applied to me with an affection of the lungs, so slight that I could hardly pronounce it more than catarrh, although it had evidently a great tendency to consumption. Being a little conversant with physic, she insisted on knowing what medicine I purposed giving her, and with what view. I candidly told her

that the object of my practice would be to prevent that mischief taking place on her lungs, which would lay the foundation of a dangerous consumption. She replied, that she wished to know whether I was of opinion that she had tubercles in the lungs, and being answered that it was probably the case, she observed, that she had been told so by the proprietor of a celecrated medicine for consumption of the lungs, whose name she mentioned; that this medicine was recommended by the first characters in the kingdom; and that the proprietor had assured her that it *radically* cured consumptions, by *dissolving* tubercles, so that they were afterwards easily coughed up. This doctrine appeared so plausible to her, that, in spite of my advice to the contrary, she took his *infallible specific*, and I, in consequence, heard no more of her for about six weeks or two months, when she sent to request my attendance. She then informed me, that the medicine had dissolved all the tubercles in her lungs, and she was afraid a portion of her lungs too. She was so emaciated, that I could not have supposed that the disease could in so short a time have made such rapid progress. Having every symptom of a speedy dissolution, I ordered her a simple mixture to quiet her cough and bowels; and, on leaving the house, I acquainted her friends with her danger, and gave my opinion that she would not survive a week. On the fourth day afterward I was apprized of her death. The thoughts of not having attended to my advice in the first instance, her friends informed me, had made her so truly miserable the last fortnight of her life, that they were of opinion, uneasiness of mind accelerated her death.

Dr. Hamilton's observations on quackery are so applicable to this case, that I have thought proper to send you a transcription of them, from his popular work on the management of children.

"Many shocking cases have occurred within the observations of the author of this work, where women have neglected pursuing with steadiness the suggestions of regular practitioners, in consequence of the false confidence they were induced to place in the dishonest promises of the discoverers of nostrums. A simple recital of the agony of such women might appear incredible. The interference of the legislature, in checking this species of robbery, is certainly required, since not only is *money stolen*, but also *life destroyed*, and that in a *way of torture*, too, which the severity of the law has never yet exercised on the most flagitious criminals." Page 68.

In this communication, I beg you will make such omissions as you may judge proper; and am,

Sir,

Your obedient servant.

March 3d, 1805.

Our correspondent has been so explicit in his communication, that we judge any further remarks from us unnecessary. We assure him that the letters shall be taken proper care of, and sincerely thank him for the promise of his observation on the present state of the medical profession, for our next number. We have received several letters from the different parts of Norfolk, respecting the first avocation and education of the inventor of the vegetable balsam, which are inadmissible; our *sole* object being the examination of their medicines, which, if we found of any merit, we should be happy to allow, notwithstanding the inventors may not have had the advantages of a regular medical education. The properties and compositions of the medicines, and not the origin and education of the proprietors, are the objects of our investigation.

Jenkinson on the use of Arsenic in Chronic Rheumatism.

To the Editors of the Medical and Physical Journal.

Gentlemen,

I have for some time intended to give you an account of the efficacy of arsenic, internally administered in chronic rheumatism; a disease which it has never before, to the best of my knowledge, been employed to relieve; and I have only been waiting for more experience, to justify my recommendation of it. Having, in the course of last summer, unsuccessfully exhausted the common list of anti-rheumatic remedies (warm bath, various liniments, antimony, Dover's powder, bark, mercury, opium, guaiacum and ammonia, in several forms and combinations; mustard seed, horse-raddish, &c. &c.) in the case of a friend of mine, who was every day becoming a greater martyr to that tormenting complaint, some analogies and reasonings, which will probably appear very loose to many of your readers, and particularly to those who do not know how casually medical discoveries are often brought to light, led me to the use of this medicine; I will slightly advert to them before I proceed.

1. Most of the common anti-rheumatic remedies, for instance, warm bathing, antimony, guaiacum, mercury, bardana, are employed in cutaneous diseases: arsenic, too, is sometimes so employed.

2. Arsenic cures certain intermitting paroxysms and pains, as ague, and head-ache: the pains of rheumatism have their periodical aggravations.

3. Arsenic is a powerful tonic, and chronic rheumatism is a disease of debility.

The sufferings of my patient, a young man of twenty-six, were altogether confined to his chest and loins for several months ; he was during this time nearly free from pain in the day, but the nightly paroxysms kept increasing in violence. His limbs at length became generally affected, and one of his knuckles much enlarged ; he was now never easy upon motion, and at last got almost unable to leave his room. His appetite and strength were much impaired, and having before tried such a variety of remedies, he despaired of his situation, and began to set himself down as a cripple for life. He had, moreover, been troubled for some years with a pretty general eruption of the herpetic kind, which first appeared after a course of mercury, and which at times was very virulent. Under these circumstances it was, that I recommended to him the use of Fowler's mineral solution. He began, as is usual, by taking four drops in water three times a day, which he gradually increased to three, and finally to four times the number ; a quantity which few persons can bear, ten or twelve drops being commonly sufficient. In two or three weeks, without the aid of any other remedy, he was considerably relieved, and by a perseverance of two months in the course, was, and still remains, perfectly cured, both of rheumatism and herpes. He began with the solution in October, and consequently recovered under all the disadvantages of wet, cold, and changeable weather. As he was completely tired of nauseous medicines before, the tastelessness of this was a great recommendation with him, as it must be with others, and contributed very much to his steadiness in its use. I have since had the satisfaction of relieving other rheumatic patients by the same means, after the failure of the usual remedies ; and Mr. Hardman (the gen-

tleman who lately gave you an account of his success with the cupping glass, in a case of abscess) informs me, that a lady under his care has lately been relieved from the most violent pains of the kind, which had set the best of the old remedies at defiance for thirteen or fourteen months, by the use of the mineral solution alone. The disease began, in this case, to yield to it in three or four days; she still continues the medicine, and has lately walked three or four miles at a time, after having been unable to move about her own house with comfort for several months. In this case too, as in the others, enlargements of the joints have subsided, and I have no doubt, from what I have seen, that this remedy, employed with proper discrimination and care, will be found worthy to supersede many others, and to afford more relief than has commonly been afforded in the worst cases of chronic rheumatism. I must be allowed to make two or three remarks before I conclude.

It will be thought by some, that the cutaneous disease led me to the use of the arsenic in the first case. I should not deem such a remark of any great consequence, if it were urged; but I will just assure you that it never occurred to me, in the first instance, to direct my attention to the removal of an eruption, which length of time had rendered familiar, and to the disappearance of which every subsequent illness might have been ascribed, as is occasionally done; and that this circumstance added but little to my previous determination to employ that solution on account of the rheumatism, though its double effect has certainly much increased the pleasure of the cure. Lest the union of two complaints in this case may lead to some suspicion of the pain differing in any respect from rheumatic pain in general, it may be worth while to observe, that in the other cases there was nothing of the kind.

Cases of chronic rheumatism, in which the usual remedies have failed, and in which the arsenic may be improper, will no doubt occur; but with your readers, I conceive particular references are quite unnecessary: most gentlemen of the profession are fully competent to form their own judgments. For the same reason, it may be superfluous to hint, that a few drops of laudanum will be occasionally required, to make the solution sit easy with delicate persons, and that a gentle laxative, in case of costiveness during its use, should not be forgotten. Henceforth, perhaps, it may be right to give the solution a trial, for reasons obvious to your readers, in some tedious arthritic cases; and likewise in the wandering pains and anomalous ailments of those syphilitic patients, who have sustained permanent injury both from the disease and its cure. It would be easy to extend the prospect still farther, but let us first cultivate the ground we have gained.

A professional gentleman, to whom I mentioned part of the facts here detailed, gave it as his opinion, that the arsenic owed its success in rheumatism *entirely* to its tonic powers. Undoubtedly it would not have so succeeded, if its powers had been of an opposite kind; but it is not the property of tonics in general *directly* to relieve rheumatic pains and reduce rheumatic enlargements; nor can we attribute to its tonic powers its alleged anodyne properties in some cancerous cases. Besides, the pain in all the patients alluded to in this letter, has yielded to it before the strength was perceptibly, or could be materially improved, insomuch, that the strength may as well be called the consequence of the ease as the reverse. That arsenic has a local, in addition to a general effect, is evident from the itching and partial discolorations so often attendant upon its use; and it is certainly to its peculiar action upon the superficial circulation, that we must ascribe much of its efficacy

in cutaneous complaints, if not its other qualities. However, I am by no means inclined to be uneasy about its modus operandi, I am only anxious about the matter of fact; and I hope, on that account, that your correspondents will turn their attention to the subject without delay: I have much deceived myself, if it be not very well worth their while.

I wish most sincerely I may soon have more success to communicate: in the mean time, I shall be particularly obliged to any gentleman who publishes an account of impartial trials made with this medicine, be the event what it may.

I am, &c.

JOHN JENKINSON.

Manchester, May 21, 1804.

Med. & Phys. Jour. vol. xi, *p.* 492.

Whitlam on Arsenic in Rheumatism.

To the Editors of the Medical and Physical Journal.

Gentlemen,

When we can snatch a fellow-creature from impending death, or even alleviate his sufferings, and render the remaining period of his existence comfortable, we feel the cheering influence of a ray emanating from the purest source of pleasure. Our gratification is more heightened, if the means made use of promise to contribute to the relief of others labouring under similar afflictions.

In compliance with the request of Mr. Jenkinson, in the 64th number of your Journal, I shall state the strikingly bene-

ficial effects of the solutio arsenici in an inveterate case of chronic rheumatism.

——— Ward, a female, who is now about the age of 35, was suddenly attacked, about two years since, with a violent pain near the middle of the spine, which hindered her from bending forwards without extreme difficulty. It was accompanied by a distressing pain in the epigastric region, attended by dyspepsia. She was pregnant at that time, and in a few days a miscarriage ensued. The pain then nearly left her; but it returned with redoubled violence in about a month. Her joints became affected, particularly her knees and ankles, which rendered walking extremely irksome; those of her fingers began to swell, and one of them became enlarged to nearly twice its proper size. She received only a temporary alleviation by the aid of remedies usually applied in such cases, though assisted by electricity.

She gradually became worse till the month of June last, when she began to take the solution, as recommended by Mr. J. In a few weeks she was much relieved, and was capable of walking without being incommoded by the pain; and since that time the swelling of her joints has decreased, and her general health is much improved.

There are two things worthy of particular notice in this case. 1st. The disease never put on the characteristic appearance of acute rheumatism. 2d. From the peculiar appearance of the countenance, and state of the pulse, tonic remedies seemed indicated; but none of them produced the effect of invigorating the system till arsenic was given. She still perse-

veres in using the medicine, and has the very pleasing prospect of a permanent cure from the use of it.

I am, &c.

JOHN WHITLAM.

Nottingham, Nov. 7, 1804.

Med. & Phys. Jour. vol. xiii, *p.* 16.

Jenkinson on Arsenic in Rheumatism.

To the Editors of the Medical and Physical Journal.

Gentlemen,

Mr. Whitlam's account, in your last number, of a case of inveterate chronic rheumatism, relieved by the use of the solutio arsenici, was of course exceedingly gratifying to me; and I wish most heartily, for the sake of those afflicted with that tedious and painful disorder, such of your correspondents as have made trial of the same remedy, would give equal publicity to their experience. What I have seen of its effects, since I published my first account in your Journal, has only tended to confirm what I there advanced; and I have scarcely a doubt remaining, but it will prove a most valuable medicine indeed, even in those states of the disease which we have hitherto been very little capable of relieving.

The lady under Mr. Hardman's care, before referred to, has been repeatedly and effectually relieved from returns of the symptoms, by the use of the solution.

William Daval, a farming labourer in Staffordshire, ætat. 60, in the course of the last summer obtained a complete cure

by the same means, from a combination of lumbago and sciatica, which had crippled him for months, and baffled the force of the usual remedies. I had the pleasure of seeing him about two months after his recovery, holding the plough with his usual strength and spirits. His thankfulness was excessive; and his expression of it was such as would have excited delightful emotions, even in the calmest of beings.

As the safety of the remedy has been established, by a long and general employment of it, in other complaints, I hope the liberal humanity of my professional brethren will prompt them to have recourse to it in chronic rheumatism, upon the earliest occasions, and to communicate the result of their experience without unnecessary delay; in all probability, the comforts, if not the existence of many fellow-creatures, now miserable, are depending upon their compliance.

I am, &c.

JOHN JENKINSON.

Manchester, Jan. 12, 1805.

Med. & Phys. Jour. vol. xiii, *p.* 525.

Roberton, on the Internal Use of Cantharides in Ulcers.

In every period of the history of surgery, the most successful plans adopted in the treatment of ulcers seem all to have acted on the same principle; and in whatever manner these various applications have been made, the substances employed have uniformly been of a stimulating quality. These have almost always produced escarotic effects, and been applied externally. Of late years, it is well known that the adhesive straps and roller have been successfully employed; and I may men-

tion, that it is alone by stimulating the parts to which they are applied, that these can produce their beneficial effects.

The natural transition from producing and supporting such a state of action by any kind of external means, to that of effecting the same purpose by the external use of such medicines as cantharides, must be evident to every one who knows any thing of the extensively stimulating powers of that drug.

It is upon this principle that all other stimuli, applied internally to sores, &c. produce similar effects, though I have found, so far as my experience goes, none of them, in their operation, so permanently effectual as that medicine. I avoided mentioning this fact at an earlier period, because I wished to mature my observations on the subject.

To those who without prejudice, or a wish to condemn, give this substance a fair trial in such complaints as may indicate its use, and who really have been unsuccessful, I have only to observe, that I can very easily account for the failure of many medical practitioners in the treatment of diseases by cantharides. It was not till after many years' experience, with the closest attention I could bestow to its operation, that I became at all successful in the removal of complaints with that medicine; and, even then, I failed in the removal of some cases, which, with greater experience in the use of medicine, I have since completely effected. It will undoubtedly be with them, as it was with myself; but by cautiously persevering in its use, they will be sensible of its importance.

CASE I.

A stout, and apparently healthy man, aged 55, about a year ago, had the middle of the sole of his foot completely perforated by a large nail. The nail was withdrawn, and the wound in a few weeks healed; but still some degree of swelling of that foot, especially towards evening, troubled him.

About four months ago, the ankle joint became somewhat more stiff than usual, and several small pustules broke out around the outer ankle, which soon formed very painful sores. In two weeks more the œdematous swelling had reached nearly to the knee, and several inches of the skin around the ankle joint had become of a dark red colour.

He now (Nov. 3, 1807) became alarmed at his situation, came to Edinburgh, and put himself under my care. The only application he had made to his sores, was simple ointment, with the constant use of a tight stocking, from which he had derived no benefit. I therefore prescribed tinct. canth. ℥ss. aq. font. ℥vi. M. a table spoonful of which was to be taken thrice a day, and the leg also to be rolled from the toe to the knee with a bandage of cotton cloth. This medicine, in sufficient doses to produce slight yet constant pain in voiding urine, with the application of the bandage, completely cured him in a month. I however advised him, on going into the country, to continue the application of the bandage for several months.

Feb. 1808. Having about a fortnight ago fatigued himself by carrying heavy burthens almost for a whole day, he was seized with pain in the formerly affected leg, and it has now become œdematous, acquired a livid colour, extending about

two-thirds up the leg, and discharging a thin watery fluid from its whole surface. I ordered him to recommence the canth. as formerly directed, and again to apply the roller.

March 1st. The discharge from the diseased surface is thick and yellow, while the œdema and livid appearance are gradually disappearing.

He having occasion to go to London, and being now pretty well acquainted with the nature of the medicine, I have desired him to continue it in absence, and to write to me if any adverse symptom occurs.

May 20th. This patient waited on me to-day, to inform me of his leg having perfectly recovered. I have nevertheless desired him to avoid all fatigue for several months, and still to wear a roller.

July, 1808. This patient has, for two or three weeks laid aside the roller, and has had no return of his complaint.

CASE II.

Nov. 6, 1806. A carpenter, aged 29 years, about twelve years ago cut his left leg with an adze. The sore has since that time been frequently skinned over, but it always in a short time after broke out again. He has applied to several medical gentlemen for relief, and, after every external application, from the mildest to the most corrosive, has been used without effect, he has for several years considered it as incurable, but while it permitted him to follow his ordinary occupation, he has been contented.

This patient has one sore near the middle of the tibia, about $2\frac{1}{2}$ inches in diameter, with thick irregular edges, discharging thin brownish matter in considerable quantity, and several other sores about the size of a sixpence surrounding it; but these have only troubled him a year, and they are neither deep nor have they thick edges, although they discharge matter equally unhealthy with the large sore. The leg and foot are swelled, and about two-thirds of its extent is of a livid colour. I have therefore prescribed tinct. canth. ℥i; aq. font. ℥vi. M. a table spoonful of which was to be taken four times a day.

18th. This patient has continued to use the cantharides in sufficient doses to produce slight difficulty in voiding urine. The discharge has become uncommonly plentiful, is of tolerably good consistence, attended with stiffness and soreness in the ulcerated parts.

30th. He has continued to take the cantharides, and now the smaller sores seem nearly healed, but there is no alteration in the appearance of the large one. The discharge from it to day, and three days past, has been alarmingly great. I even believed it impossible for such a discharge to proceed from an ulcer of these dimensions, and, suspecting sinus, I minutely examined the whole of its surface with a probe, but could discover none; the cantharides were therefore continued.

Dec. 7th. All the smaller sores were completely healed, but there was no alteration of the larger one. This day he became impatient, and took a very large dose of cantharides. About an hour afterwards he became sick and vomited, and during the night voided brown bloody matter along with his urine. He informed me of this circumstance, and I advised him to take a smart cathartic and use warm applications above

the pubes. The cathartic was rejected by vomiting, and indeed, for two days, nothing would remain on his stomach, and it was not till after the most excruciating pain, that these symptoms began to abate. The sore has now become so irritable that he cannot even allow the weight of the common dressing to press on it.

17th. The above symptoms have disappeared, but the sore does not seem to have been much affected by what has been done.

20th. This day I began to use a solution of the muriate of mercury as a wash, and desired him to recommence the cantharides, none of which he had taken since the 7th.

29th. The sore now began to have a very favourable appearance, the whole surface looking florid, the edges becoming thinner, and its diameter not being so great as formerly. The cantharides and solution were therefore continued.

Jan. 20, 1807. The sore now evidently contracted in size. The solution was applied where the edges seemed not disposed to heal, and this produced the most beneficial effects. The cantharides were still continued.

March 25th. The sore remained nearly in the same state since last report, although he had taken the cantharides in sufficient doses to affect his urinary organs.

April 6th. The sore now assumed a black unhealthy appearance, and became thicker in the edges. He had continued to take his medicines regularly, and in every respect to obey my prescription. My patient could only account for this

change of appearance from his having about three days ago exposed himself to very severe fatigue for a whole day; since which time the sore had gradually degenerated.

May 20th. Although he had taken the cantharides regularly, and in sufficient doses, since last report, the sore had still continued to degenerate, and it was now larger than I ever saw it. I certainly would now have omitted the use of the cantharides for the present, had it given him any uneasiness at the stomach, or seemed to affect his pulse in an unfavourable way, but as all went on well, I desired him to continue.

26th. The sore has again assumed a more healthy appearance, and discharges thicker matter.

August, 1807. He had regularly continued the cantharides since last report, and his leg was now perfectly well, and he himself able to work at his trade.

July, 1808. This patient still remained perfectly well.

☞ Several other cases, strongly in favour of the cantharides, are detailed, but the above are deemed sufficient to call the attention of the medical practitioner to it.

EDITOR.

A letter on the subject of the Gout, from HENRY HILL (*grandfather to the late* HENRY HILL, *Esq. of Philadelphia*) *to* MARGARET PRESTON, *ancestor of the* MOORE *family.*

The 7th of 12*th mo.* 1737–8.

DEAR COUSIN,

When I think of thy having the gout, I don't know whether I ought to be most concerned for thy present pain, or pleased with the thoughts of the benefits a valuable part of the creation will receive from the effects thereof, since I really believe, if prudently managed, it is a means to procure reason unclouded, and to prolong life; and how great a blessing that may prove in a person of thy capacity and merit, not only to thy own hopeful issue immediately under thy tuition, but to all that have the happiness of thy conversation, requires a more elegant pen than mine to describe. I heartily wish thou hadst an easier game to play that might have the same good effect; but since complete happiness is not to be expected in this world, and that the frailty of our natures obliges our lives to be checkered with afflictions, I scarcely know what distemper I would accept in lieu of the gout were it in my power, and I think it seldom admits of a competitor. It indeed often, especially towards the decline of life, changes its seat, as sometimes to the bowels, and takes the share of a diarrhœa, other times on the lungs, and proves a troublesome cough, &c. all which I think may be easily remedied by diet and medicines, though I believe medicines to have the least share, even in a palliative cure of the gout, at which I only pretend to endeavour; for as to a perfect cure, under a libertine life, I don't think there's any person of common understanding will be so hardy as to pretend to, or if such an one should be, I'm persuaded he'd find but few proselytes.

Be pleased then to hear what a person of upwards of 39 years painful experience, without above six months successive intermission (except by diet) in all that time, has to say, in order to render life tolerable; but previous to my method of living which I use, it must be considered there is abundance of difference in constitutions, in custom of living, and between youth and age, and that what agrees with one mayn't with another, and when it is found disagreeable I should advise to quit it directly, for fear it should prove fatal.

I am of a thin lean body, a constipated bowel, and choleric constitution, and was a great lover of strong drink, though soon satisfied, except for company sake, by means of which I have been sometimes induced to take too much; but then I dearly paid for it, scarcely being able to get myself cool again in a week. A pretty hearty feeder (rather too voracious), and am now in the 66th year of my age.

About the 45th of my age, and 19th of the gout, I was scarcely clear of the gout three months together, and could hardly walk a quarter of a mile, when, by gradation, I brought myself to an entire milk diet, which agreed very well with me for near seven years, and of which I drank (for I took it no other ways) three or four quarts per day, raw. If it proved feverish I mixt it with water, or omitted quite, and lived a day or two on bread and water, and the feverish indisposition would go off; then I would confine myself to my milk again, and had little or no sense of hunger, but very thirsty, which my milk mostly satisfied, but when it would not fair water would. In about four months time I could walk five or six miles an end, and pretty well.

I held this method near seven years, and had but one fit of the gout, and that was by drinking about a pint and a half of punch, &c. In the autumn of the seventh year the quartan ague seized me (which I never had before), upon which I was obliged to quit my milk, and live otherwise, but low, and take the bark. When recovered, took to my milk again, and continued it till the latter end of the next summer, when a diarrhœa seized me, which had like to have carried me off, so that in short I was obliged to leave off my milk diet, as being too cold and slippery, and breeding too viscid a nutriment, and gradually falling on my old way of living, only very sparingly of strong drink, and the gout came as formerly, but not altogether so violent as when I indulged myself with strong drink.

But since I have been conversant with Dr. Cheyne's works, I have brought myself pretty near to what he calls his trimming diet, but with an entire abstinence from strong drink, which yet I think might be allowed to colder, moister constitutions, to not above one glass though, after an animal food dinner, but by no means soon after taking of milk, and I think I never enjoyed a steadier state of health since I can remember. I have a pint of milk boiled at night, of which I drink half as hot as I well can, the other half a little before I go to bed; in the morning my breakfast is the like quantity ordered the same way, and my dinner is a pint of milk raw, with a good deal of well-baked light wheaten bread, no matter how coarse. This I find sufficient for me, though I suppose a younger person might very well take twice the quantity without any prejudice. Thus I live alone; but, if I have any company, I take bread and butter, with coffee or tea, besides my allowance of milk at breakfast, and a light dinner of any young animal food with vegetables, and glad of the opportunity.

Moderate exercise I find very beneficial, especially in clear weather, not too windy, as it gives a jog to the fluids, and a tensity to the solids, without much expence of spirits, of which if a dejection should happen (from which I believe few of either sex are clear scarcely one week together), take spirit of lavender, sal. volatilis, tincture castor, of each 20 drops in a glass of water and sugar, which I find generally relieves. If too costive, use much greens at dinner, and take two tea spoonfuls of tinctura sacra, or one of elixir proprietatis, in sugar and water, in the morning fasting, and repeat as need require. This is excellent to take off sickness at stomach too.

If too laxative, forbear all greens, put one-third water to the milk, and boil very well together. If a troublesome cough affects, as sometimes it does, take six or eight drops of liquid laudanum at going to bed, every night, &c. This is the method I use with pleasing effects hitherto, and if it will regulate the gout so that I have but one mild fit a year, I shall think it a great happiness, and I should be glad if this or any thing else that lay in my power could be serviceable to thee.

I remain thy obliged loving friend and kinsman,

HENRY HILL.

Extract of a letter to the Editor, from Dr. WILLIAM FOUSHEE, *junior, corroborative of the efficacy of Vaccination, dated Richmond* (*Vir.*), *Nov.* 29, 1808.

Our city has been in great alarm at a man arriving from New York with the small-pox. As soon as it was known, he was removed to our hospital, where I attended. Six others, who were exposed to the infection from him, were likewise re-

moved to the hospital, and were inoculated for the small-pox: three of those last sent had the cow-pox, and did not receive the small-pox, while the others did: another instance of the preventive of cow-pox against the small-pox. Besides inoculating them, I made them expose themselves in every way they could.

Dancer's Account of a Hermaphrodite Goat, in a letter to the Editor, of March, 1808.

In my last letter to you, if I recollect rightly, I mentioned that I had, in a young goat, a singular instance of hermaphrodite construction. The testicles, without any proper scrotum, were lodged under the skin, close to the navel, and anterior to the penis, which showed itself in the usual place, with a prepuce. In the perineum, at the root of the penis, there was a strong representation of a vulva; but it was closed, having only a small aperture from whence the urine flowed, the penis having no urethra.

The animal was killed for dissection, but I cannot give any satisfactory account of what was ascertained by it. The external parts were, however, preserved, and I now send you the dry preparation, exhibiting every thing above described. Such examples of lusus naturæ may at least excite curiosity, and may casually serve to throw light on some abstruse points in physiology. This animal was undoubtedly intended for a male; for, notwithstanding the mal-conformation of parts, which made it impossible for him to copulate with a she-goat, he was a very troublesome rutter; insomuch that an attempt was made by some person to castrate him, which you will see by the cicatrix remaining.

Account of farther discoveries in extracting Metals from the Earths and Alkalies.

The Swedish chemists, M. PONTIN and BERZELIUS, have succeeded in obtaining amalgams of the metals of lime and barytes, of magnesia and of strontites, by exposing these earths, moistened to negatively electrified mercury; but this method has no effect on alumen or silex. These gentlemen have also made a very interesting experiment on ammonia: they placed a solution of it in contact with mercury, negatively electrified, as in the former experiments, and thus succeeded in obtaining a soft amalgam, which proves its metallic nature.

Mr. Davy has since improved on the discovery of the Swedish chemists. The amalgam of barytes and mercury obtained by their method was distilled, and the mercury being driven off, he obtained the metal of barytes in a pure form; and by the same process he procured the metals of strontites and magnesia nearly pure. The earths are mixed with red precipitate, which is negatively electrified; the amalgam is absorbed by fresh mercury, and, when it becomes semi-fluid, is distilled in the vapour of naphtha in a tube of plate glass.

The above facts were communicated by Mr. Davy to the Royal Society a few weeks ago, when he also stated, that in experiments made with a voltaic battery of 36,000 square inches, on barytes, strontites, and the other alkaline earths, and on silex and alumen, all these bodies, when slightly moistened, and acted on by iron wires negatively electrified, undergo changes at the points of contact; and the metals of the earths appear to form alloys with the negatively electrified iron.

Mr. Davy likewise metallized the earths by electrifying them when mixed with the oxyds of lead, of silver, or of mercury, when the metals of the earths and the common metals were revived together in the state of alloy.

Since Mr. Davy's first discovery of the metallic nature of the alkalies by the voltaic battery in last November, several others have been maſe relative to them and the earths, with a rapidity seldom witnessed before on similar occasions. Soon afterwards the discovery of the French chemists was made, that the metal of potash might be made in large quantities without the aid of galvanism, by means of the superior attractions of iron and charcoal for oxygen in a strong heat, and the tendency of the new metal to form an alloy with iron; and in the foregoing short notice are contained no less than four more important discoveries; that of the Swedish chemists, of the effect of negatively electrified mercury in accelerating the metallization of the earths; and those of Mr. Davy, of the obtaining these metals pure, by the distillation of the amalgams so formed; of the operation of the oxyds of lead, silver, and mercury, when electrified, in reviving the metals of the earths by forming alloys with them, and of the same effect produced by negatively electrified iron to the metallic state.

The discovery of ammonia being of a metallic nature, shows that is probable the very air we breathe contains metal in a gaseous state; for azote, which forms a large portion of our atmosphere, has long since been proved to be a component part of ammonia.

Silex and alumen having both proved refractory to the processes which obtained metals from the other earths, gives additional weight to the old opinions of their being of the same

nature, or of their being different modifications of the same substance. *Athenæum, Sept.* 1808.

Davy and Singer's Chemical Experiments.

It has been asserted by most writers and experimentalists, that silver burns with a bright emerald-green light. In Mr. Davy's late lectures at the Royal Institution, the deflagration of silver leaf was attended by the emission of a brilliant white light, which the professor ascribed to the great purity of the silver employed; and he expressed an opinion that the green flame usually observed, arose from the admixture of copper with the silver. Mr. G. B. Singer has, however, discovered that this phenomenon proceeds from a different cause. Having observed that Mr. Davy's conducting wires were terminated by charcoal, he repeated the experiment with a similar arrangement; and applying the charcoal to a pure silver leaf, it immediately burned with a beautiful white light. Some of the same portion of silver having been before employed when the green flame was produced, it became evident that the white light in this and in Mr. Davy's experiment proceeded from the charcoal: and that this was really the case, appeared from the immediate evolution of green light, when the contact was made by a metallic wire. By the application of charcoal to the extremity of a wire, so bent that either the wire or the charcoal may touch the silver at pleasure, the white and the green flame may be alternately produced; and a conclusive demonstration of the fact, with a pleasing variation of a brilliant experiment, will be thus at once afforded.

New York Med. & Phil. Journ. & Rev.

Account of a remarkable Case, in which a considerable Part of a Female Body was converted into Fatty Matter. By Mr. Mansfield, *Surgeon, Thrapston, Northamptonshire.*

The curiosity of the philosophical part of mankind has been of late much excited, by the accounts which we have received, of human as well as other muscular flesh being converted into fat by slow putrefaction. I have now an opportunity of adding to the stock of information on this subject, a remarkable case, which I shall describe, and which you will probably think deserves to be recorded as an extraordinary one; and perhaps a similar instance may not soon occur again, in which the period of time consumed in the process can be so well ascertained. The examination of it afforded me great gratification; and I have that reason at least for conceiving, that the account of it may be acceptable to many others.

A young woman of the parish of Islip, near Thrapston, in Northamptonshire, seventeen years of age, of a middle stature, and thin in her make, was drowned in the river Nen, near that place, upon the 1st of December, 1798; and the body was not discovered until the 27th of November, 1799, at a place about four miles off, by land; and, from the circuitous course of the stream, about as far again by water.

As soon as I heard that it was found, I lost no time in going to see it, glad of an opportunity to satisfy myself, in ascertaining the truth of what had been said upon this subject, viz. that muscular flesh, immersed in a stream of water, will in time become a fatty substance, not subject to further decay in water, nor to the usual effects of putrefaction.

I found the fact fully proved: the parts subject to this change had the appearance, as it first struck me, of a lump of fat. The bones of the head, neck, and ribs, were nearly cleared of all the soft parts, the chest was quite void of its contents; the ribs themselves as clear of flesh as possible; but from thence downwards, all was firm and perfect. The abdomen and its contents remained in a natural state; as did the thighs and legs; and all had the same external appearance with a thick covering of fat, cracked all over like the peel of a walnut when ripe.

There was nothing that had any resemblance to skin, but an upper stratum about half an inch deep, which was disposed to separate from the muscular parts; this I took at first to be adipose membrane very much thickened, as it was of a loose and friable texture; but it is probable that this was skin, which had undergone the same change. Under this was a firm and white stratum of fat, very considerably thicker than the former; this appeared to be the upper part of the muscles, as it was not separated from the under part, which latter was pale, firm, and attached to the bone, as in a fresh state; upon cutting into this last substance, however, I found it very offensive (though the upper part was not so) to the smell.

Not any of this substance was crystallized like spermaceti; all the remaining part of the body had the exact appearance externally of a boiled ham, when stripped of the skin, both as to colour and substance.

That the putrefaction in this case was slow, is easily accounted for, as the body was in the water all the preceding winter, which was a very cold one; the summer too was uncommonly cold and rainy; and, on this latter account, the

stream unusually rapid. From some accidental cause, the body had been detained all the time under the surface of the water, perhaps by the clothes having been entangled by the roots of a tree.

That there could be no doubt of this being the same body that was lost at the above period, it may be proper to observe, that the shoes and buckles remained on her feet, which the parents immediately identified.

It may be difficult to determine, at what precise period of time the change began to take place, and how long it would have been before the remainder of the muscular flesh would have undergone the same alteration; but it is clear, that the process of converting a considerable part into fatty matter of a combustible nature, may be completed in the period of twelve months; which may suggest a hint as to the propriety of making this change in the flesh of horses, &c. an article of manufacture.

I have been much gratified by this investigation, and in having it in my power to give an account of it to those, whose curiosity, like mine, may have been excited by this extraordinary discovery, respecting the change taking place in animal matter, from slow putrefaction.

S. M. Mansfield.

Thrapston, *Northamptonshire*, *Dec.* 7, 1799.

Med. & Phys. Jour. vol. iii. *p.* 10.

Method of rendering Alkalies Caustic.

The method of *rendering alkalies caustic*, adopted by Prof. Trommsdorf, is, on account of its facility, worthy of our attention. To the boiling alkaline solution, he gradually adds finely powdered quick lime, till the whole is perfectly saturated. The liquor is then filtered, and submitted to the usual mode of treatment. The advantage of this process is, that the lime does not so readily effervesce as when slacked in water, so that the separation of the alkaline ley is much more easily effected.

Pharmaceut. Experimental Chemie, p. 146.

Account of a new Poultice, lately proposed by Mr. T. Payne, *Surgeon, Brook-street, London.*

From the second edition of a small pamphlet, addressed by Mr. Payne to the faculty, we extract the following remarks: " It may appear strange," says the author, " to those who do not enter into the minutiæ of surgery, that I should look upon the proper application of so simple a remedy as that of poultice, as an essential object (which certainly I do, as well as many other (*deemed*) little things in surgery); but having met with repeated proofs of the necessity of it, I have decidedly formed my opinion on this head. What gave me the first idea respecting the subject of this publication, was, that in the Parish Infirmary of St. George, Hanover-square, to which I have the honour of being surgeon, frequent complaints were made by the patients under my care, that the poultice was uneasy and caused much irritation: this, I supposed, arose

from the circumstance of its not being made of bread so good as usual (an alteration having been made in that article in the house), on which account I thought it my duty to remedy it, if possible, for the ease of the poor afflicted patients. I immediately set about making experiments, many of which I need not mention, as I have formed a poultice which succeeds beyond my most sanguine expectations, and I flatter myself it may prove of some utility. The composition will not only save full half the expence of the former poultice, but the consumption of bread corn entirely; a most desirable object to accomplish in the present scarcity of that article; which affords me infinite pleasure, and I make no doubt will prove a stimulus (if any be necessary) to those gentlemen into whose hands this may fall, to induce them to give it a fair and impartial trial. The particulars and expence attending the late and present poultice in use at the aforesaid infirmary, per week, are as follow:

"OLD POULTICE.

	s.	d.
Bread 49lb. at $2\frac{1}{4}$d. per pound -	10	$2\frac{1}{2}$
Milk 14 quarts, at 2d. per quart -	2	4
Lard $2\frac{1}{4}$lb. at 9d. per pound - -	1	$8\frac{1}{4}$
	14	$2\frac{3}{4}$

"NEW POULTICE.

	s.	d.
Fine pollard $3\frac{1}{2}$ pecks, at 6d. per peck	1	9
Genuine linseed flour 14lb. at 4d. per lb.	4	8
Lard, a quarter of a pound - - - - -	0	$2\frac{1}{4}$
	6	$7\frac{1}{4}$

STATEMENT OF DEATHS,

WITH THE DISEASES AND AGES,

In the City and Liberties of Philadelphia, from the 1*st of January*, 1808, *to the* 1*st of January*, 1809.

DISEASES.	Under 2 years.	From 2 to 5.	From 5 to 10.	From 10 to 20.	From 20 to 30.	From 30 to 40.	From 40 to 50.	From 50 to 60.	From 60 to 70.	From 70 to 80	From 80 to 90.	From 90 to 100.	From 100 to 110	Ages unknown.	Total.
Abortion	0	0	0	0	0	1	0	0	0	0	0	0	0	0	1
Abscess	0	2	3	0	0	0	0	1	1	0	0	0	0	1	8
Angina pectoris	1	0	0	0	0	0	0	0	0	1	0	0	0	0	2
Aneurism	0	0	0	0	0	0	1	0	0	0	0	0	0	0	1
Anthrax	0	0	0	0	0	0	0	1	0	0	0	0	0	0	1
Apoplexy	0	0	0	1	5	4	6	3	9	3	1	0	0	2	34
Asthma	0	0	0	1	0	1	1	0	0	1	0	0	0	0	4
Atrophy or marasmus	20	1	1	0	0	0	3	1	2	0	1	0	0	0	29
Burns	4	4	2	0	0	1	1	0	0	0	0	1	0	2	15
Cancer	0	0	0	0	0	1	5	1	1	1	0	0	0	0	9
Casualties	1	0	0	0	1	2	5	1	1	0	0	0	0	1	12
Catarrh	19	0	0	0	0	0	2	0	0	1	0	0	0	1	23
Cachexy	0	0	0	0	0	0	0	1	0	0	0	0	0	0	1
Caries of the spine	0	0	0	0	0	1	0	0	0	0	0	0	0	0	1
Chlorosis	0	0	0	0	1	0	0	0	0	0	0	0	0	0	1
Constipation	0	0	0	0	1	0	0	0	0	0	0	0	0	0	1
Cholera morbus	192	20	4	1	0	1	1	1	0	0	0	0	0	10	230
Cholic	3	0	0	1	3	1	2	1	1	3	1	0	0	1	17
Consumption of lungs	12	10	10	21	67	73	41	33	12	1	1	0	0	20	301
Convulsions	100	12	6	0	8	6	4	1	2	0	0	0	0	6	145
Contusion	0	0	0	0	0	3	0	0	0	0	0	0	0	1	4
Debility	10	1	0	1	1	0	1	0	0	0	1	0	0	0	15
Decay	19	4	2	4	4	6	9	9	6	5	2	1	0	1	72
Diabetes	0	0	0	0	0	0	0	2	0	0	0	0	0	0	2
Diarrhœa	20	10	1	4	2	6	8	8	9	4	2	0	0	0	74
Dropsy	1	3	1	2	7	16	11	9	8	5	2	0	0	2	67
Dropsy of the brain	31	8	3	2	3	4	0	1	0	0	0	0	0	0	52
Dropsy in the chest	0	0	0	0	2	2	4	3	2	2	1	0	0	2	18
Drowned	0	0	1	5	5	3	6	1	2	0	0	0	0	5	28
Disease of the heart	0	0	0	0	1	0	0	0	0	0	0	0	0	0	1
——— hip joint	0	0	1	0	0	0	0	0	0	0	0	0	0	0	1
Drunkenness	0	0	0	0	0	3	0	0	1	0	0	0	0	1	5
Dysentery	21	8	2	2	0	1	1	0	2	1	0	0	0	2	40
Dyspepsia	0	0	0	1	0	0	1	0	0	0	0	0	0	0	2
Dyspnœa	2	0	0	0	0	0	0	1	0	0	0	0	0	0	3
Eruptions	2	0	0	0	0	0	0	0	0	0	1	0	0	0	3
Epilepsy	1	1	0	2	1	0	3	4	0	0	0	0	0	0	12
Erysipelas	3	0	0	0	0	1	0	0	0	0	0	0	0	0	4
Executed	0	0	0	0	2	0	0	0	0	0	0	0	0	0	2
Fever, type not ment.	4	0	2	3	2	3	4	0	1	2	0	0	0	2	22
——— bil. remit. & inter.	5	0	1	4	13	7	7	2	3	1	0	0	0	0	45
——— typh. nerv. & putr.	0	0	3	7	13	7	3	1	1	0	0	0	0	0	35

CONTINUED.

DISEASES.	Under 2 years.	From 2 to 5.	From 5 to 10.	From 10 to 20.	From 20 to 30.	From 30 to 40.	From 40 to 50.	From 50 to 60.	From 60 to 70.	From 70 to 80.	From 80 to 90	From 90 to 100.	From 100 to 110.	Ages unknown.	Total.
Fever, inflam. phrenitic, and cephalic	1	0	2	2	0	2	3	0	0	0	0	0	0	0	10
Fever, scarlet	0	0	0	2	0	0	0	0	0	0	0	0	0	0	2
—— hectic	2	0	0	0	1	0	1	0	0	0	0	0	0	0	4
—— puerperal	0	0	0	0	1	0	0	0	0	0	0	0	0	1	2
Fracture	0	0	0	1	0	2	0	0	0	0	0	0	0	0	3
Gout	0	0	0	0	0	1	1	1	0	1	0	0	0	0	4
Gravel	0	0	0	1	0	0	0	0	0	0	0	0	0	0	1
Hives	38	10	3	0	0	0	0	0	0	0	0	0	0	2	53
Hæmorrhage, hæmorrhoids, & hæmoptysis	0	1	0	0	4	1	2	1	1	0	0	0	0	1	11
Hooping-cough	10	1	0	0	0	0	0	0	0	0	0	0	0	0	11
Inflam. of the brain	5	2	2	0	3	2	3	2	8	0	0	0	0	0	22
———— lungs	24	5	4	0	10	4	5	3	5	4	0	0	0	2	66
———— liver	4	1	2	0	1	7	1	4	2	1	0	0	0	2	25
———— stom. & bow.	13	0	0	3	1	3	5	1	3	2	0	0	0	2	33
———— peritoneum	0	0	0	0	1	0	0	0	0	0	0	0	0	0	1
Insanity	0	0	0	0	7	8	2	3	2	1	0	0	0	2	25
Jaundice	2	0	0	0	0	1	3	1	1	1	0	0	0	0	9
Lethargy	0	0	0	0	0	0	0	1	1	1	0	0	0	0	3
Locked jaw	1	1	3	0	0	0	3	0	0	0	0	0	0	0	8
Measles	50	13	8	0	0	0	0	0	0	0	0	0	0	2	73
Mortifica. and gangrene	5	3	1	0	0	0	4	3	1	1	0	0	0	2	20
Old age	0	0	0	0	0	0	0	0	0	16	15	5	0	0	38
Overlaid	1	0	0	0	0	0	0	0	0	0	0	0	0	0	1
Palsy	0	0	0	1	0	0	4	6	2	1	2	0	0	1	17
Parturition	0	0	0	0	3	0	1	0	0	0	0	0	0	0	4
Pleurisy	4	0	1	4	6	6	6	3	3	1	0	0	0	1	35
Quinsy	0	0	0	0	0	0	0	1	1	0	0	0	0	0	2
Rickets	1	0	0	0	0	0	0	0	0	0	0	0	0	1	2
Rheumatism	0	0	0	0	0	0	1	1	1	0	0	0	0	0	3
Scrophula	0	1	0	0	3	0	0	0	1	0	0	0	0	1	6
SMALL-POX, natur.	36	32	26	15	12	10	2	1	0	0	0	0	0	7	141
———— inoculated	1	2	0	1	0	0	0	0	0	0	0	0	0	0	4
Sore throat	4	1	0	1	0	0	1	0	1	0	0	0	0	0	8
Still-born	126	0	0	0	0	0	0	0	0	0	0	0	2	0	126
Suicide	0	0	0	0	0	1	0	1	0	0	0	0	0	2	4
Sudden death	4	0	4	0	6	15	6	7	4	0	0	0	0	4	46
Syphilis	2	1	0	1	9	0	2	1	0	0	0	0	0	0	16
Teething	10	0	0	0	0	0	0	0	0	0	0	0	0	0	10
Thrush	7	0	0	0	0	0	0	0	0	0	0	0	0	0	7
Tumors	0	1	0	0	1	0	0	0	0	0	0	0	0	0	2
Ulcers	3	0	0	0	0	0	0	0	0	0	0	0	0	1	5
Visceral obstructions	0	1	0	0	0	0	0	0	0	0	0	0	0	0	1
Worms	11	5	2	0	0	0	0	0	0	0	0	0	0	2	20
Wounds	0	0	1	0	0	0	0	0	0	0	0	0	0	1	1
Diseases unknown	31	2	0	1	1	2	0	1	1	0	0	0	0	3	43
Total	867	167	98	95	212	219	[illegible]	128	[illegible]	61	30	7	2	101	2271

DEATHS IN EACH MONTH

OF THE ABOVE PERIOD.

	Adults.	*Children.*	*Total.*
January - - -	91	45	136
February - - -	73	50	123
March - - -	91	63	154
April - - -	96	73	169
May - - -	81	98	179
June - - -	95	132	227
July - - -	111	263	374
August - - -	109	188	297
September - - -	88	97	185
October - - -	71	83	154
November - - -	81	71	152
December - - -	59	62	121
Total - - -	1046	1225	2271

By order of the Board of Health,

WILLIAM NESBITT, *Clerk.*

Health Office, Jan. 29, 1809.

Alderson's account of a Lusus Naturæ of the Mammæ.

Mrs. ——— informed me she had not been able to give suck to her three last children, because, when the draught came into her breasts, the milk flowed out under her arms. On examination I found the mammæ small, and under the fold or lamella of the pectoral muscle on each side, in the fore part of the axilla, a large expanded gland, or small mamma, perfectly well defined, with a head or nipple like a small wart, formed by the tubi lactiferi; three of which were very distinctly observable upon pressure, producing a stream of pure milk the size of a crow quill.

These glands uniformly became tumid, and filled at the same time with the mammæ, and as uniformly poured out the secreted fluid before the breast suffered any considerable distension.

Whenever the child was put to the breast, the milk ran in a constant stream from these glands.

She did not observe this peculiar conformation till after her second lying-in.

Med. & Phys. Jour. vol. iii, *p.* 402.

New Association.

It is proposed by a number of medical gentlemen of Philadelphia and other parts of the continent, to institute a society in the city of Philadelphia, which shall have for its object the accomplishment of a certain and facile mode of intercourse and correspondence, between all the medical characters of the United States, each with each, and with the society as a body. When it is considered that, in America, the field for medical inquiry is extended over an extent of country, equal in magnitude to perhaps one-third of the habited portion of the earth, and that of this a superior portion is as yet untouched by the labours of philosophic inquiry; and that nature, eternally diversified, changes her aspect in every climate, in obedience to almost imperceptible shades of difference or variety; and that nothing can so much promote the knowledge of her laws and operations, as the careful observation of individuals diversified by situation and their favourite objects of pursuit, minutely collated and compared; and when we reflect on the importance of collision of opinion and diversified investigation, as a correction to the errors and mistakes of unassisted

and individual inquiry: impelled by these considerations, it is confidently presumed, that no physician who feels a just interest in the advancement of his profession, and in a liberal dissemination of its important truths and practices, will rest indifferent as to the completion of the proposed association. It is certainly directed to the most important uses; and, while it does credit to the humanity and benevolence of its projectors, is eminently calculated for the equal and liberal distribution of physical science, to every quarter of this extensive continent; a desideratum never to be enjoyed by personal commerce, and only attainable through the agency of an association like the one proposed. It is expected that a meeting will be held in the city of Philadelphia, in the month of April next, for the purpose of adopting a constitution, electing the necessary officers, and enacting such laws as may then be deemed most expedient for carrying into effect the objects proposed.

Philadelphia, March 25th, 1809.

Prize Medals.

Extract of the minutes of the Philadelphia Medical Lyceum, February 22d, 1809.

The committee appointed by the Medical Lyceum of Philadelphia, to examine the different dissertations submitted for the medal offered by the society, having reported that, in their estimation, the question proposed was still left in much uncertainty, though several of the essays were eminently distinguished by ingenuity and patient research, the Lyceum came to a resolution, at a meeting on the 13th inst. to hold out the same subject for further investigation.

The Lyceum, therefore, again announce, that they will give a *gold medal* of the value of *fifty dollars*, for such an essay as may receive their approbation on the following question:

"Does the surface of the human body absorb any substance, *nutritive*, *medicinal*, or *poisonous*?"

The society requires:

1st. That the dissertations be written in the English language, and that they contain experiments widely diversified, and of a pertinent, original, and decisive kind.

2d. That the dissertations be transmitted, post paid, to their corresponding secretary, so that they may reach him on or before the first of December next.

3d. That with each dissertation the author's name be sent in a separate enclosure, not to be opened by the society, unless the medal shall be awarded to the dissertation.

That the authors of the dissertations be kept secret. In case any one of them shall disclose, or cause his name to be disclosed, the Lyceum will discard the essay from their adjudication.

By order of the society,

J. C. Rousseau, M. D.

Corresponding Secretary.

⁂ Editors of newspapers, medical or other periodical works, are requested to promote the publicity of this, through the medium of their several publications.

Prize Medals, offered by the Humane Society of Philadelphia.

AT a meeting of the managers, on the eight instant, the decision of the medical professors of the University of Pennsylvania was received, containing information that they have carefully examined the three dissertations for the prize medals, and that "they are unanimously of opinion, that neither of them are entitled to the medals, as they do not appear to contain any *original* observations;" at the same time remarking, that they are by no means destitute of merit as to arrangement and style.

The managers adopted their decision, and are again induced to offer,

For the best dissertation on the means of restoring to life, persons apparently dead from drowning, *and more effectual than any yet in use*, a GOLD MEDAL, value ONE HUNDRED DOLLARS.

For the second best, a PIECE OF PLATE, value FIFTY DOLLARS.

The dissertations to be sent to the secretary of the society (post paid) by the first of January, 1810.

They may be written in the English, French, or Latin language; to be accompanied with a sealed paper, containing the author's name and place of residence, which is not to be opened, unless the prize is decreed.

They shall be submitted to the judgment and decision of the medical professors of the University of Pennsylvania.

Although the first effort in this truly important research has not proved successful, yet, when we consider its magnitude, it is believed no additional reason need be given for continuing the attempt to enlarge our knowledge, where the preservation of human life is the object. We hope no discouragement will arise from the failure of the late dissertations, in which ingenuity and learning are evident, as our aim is *to arrive at a knowledge of the means of restoring life more effectual than any yet in use;* being persuaded that the discovery of such means will amply repay the most laborious investigation, and form a new epoch in the progress of humanity and science.

By order of the managers of the Humane Society.

Isaac Snowden, Jun. sec'y. JOSEPH CRUKSHANK, *pres.*

Philadelphia, June 15th, 1808.

*** The editor proposes to devote an occasional page of the Museum for preserving from newspapers and other sources, such advertisements of medicines, &c. as shall seem worthy of record, for the benefit of posterity. With this view he commences with one from the Medical Observer, which is there preserved "as a specimen of a particular set of advertising doctors."

TO THE AFFLICTED.

DR. M'GHIE,

No. 6, *Richmond-Buildings, Dean-street, Soho,*

"Cures all complaints incident to the human frame, whether in mind, body, or limbs; and in order that the afflicted may not despair, or be at a

loss where to apply, he calls their attention to the following extraordinary cures, many of which were accomplished when given over by the faculty.

"In 1790, Anna, the daughter of James Conner, Broadway, Westminster, was restored to life, though dead to all appearance six hours; Thomas Clark, Orchard-street, Westminster, had the dead palsy, and a great defect in his sight and hearing, twelve years and a half, was restored in twenty-seven days; Thomas Rose, Orchard-street, Westminster, was cured of a five years' consumption in fourteen days; Anthony Hartshorn, servant to Mr. Whitbread, was lame seven years with a white swelling and contracted muscles, one leg three inches and a half shorter than the other, cured in thirteen days, in 1790; Mr. Benyon, Windsor Court, Little Knight Ryder-street, Doctors' Commons, broke his knee-pan to pieces, was attended by three surgeons sixteen weeks, and pronounced incurable, was cured in eleven days; Mrs. Puller, No. 3, Queen-street, Edgware Road, cured of a dangerous canker in her mouth; Thomas Clark, No 1, Chatham Gardens, City Road, cured of a seventeen years' consumption in twenty days; Joseph Clark, No. 2, Little Sutton-street, Goswell-street, had a leprosy on him twenty years, was obliged to wear gloves constantly, cured in three weeks; Mrs. Ulkes, Whitecross-street, Cripplegate, was a long time afflicted with an abscess on her liver, was restored to health in thirteen days; James Newman had a windy dropsy for two years and a half, was restored; Mrs. Miller, the Bell Inn, Highgate, had a bloody urine, and a complication of complaints, cured in a fortnight; Mr. Simpkin, sugar-baker, Charlotte-street, Whitechapel, had a son cured of the king's evil, and another of the falling sickness; John Donovan had the falling sickness twenty-eight years, cured in a month; Mr. Graham, jun. No. 144, Old-street, had the dead palsy between three and four years, cured in a month; Mr. Newman, No 38, Red Lion-street, Clerkenwell, cured of a dangerous consumption in eleven days; Mr. Robinson, Clayton Place, Kennington, cured of a fifty years' bleeding cancer; Mrs. Ferguson, Little Catharine-street, Strand, cured of a stone cancer in her breast; and many hundreds labouring under various afflictions have been restored by the doctor's invaluable *Balsam of Life*, six bottles of which will cure a consumption of twenty years. A certain complaint cured in a few days, *without mercury*, or confinement from diet or business.

"N. B. The Doctor may be consulted at any time between the hours of 9 in the morning and 9 at night."

MEDICAL AND PHILOSOPHICAL
REGISTER.

VOL. VI.....No. II.

FOREIGN AND DOMESTIC.

Observations on American Medical Practice.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

I HAVE been a constant reader of the London Medical and Physical Journal from the commencement of its publication, and an occasional contributor to its contents; proofs that I have hitherto approved of the mode of conducting it.

It is with no small degree of pain, that I now hear frequent complaints among my medical acquaintance, of a want of selection of cases of cautious practice, and that admittance is too frequently afforded to dangerous trans-atlantic communications. Not that I would interdict, gentlemen, the reception of *foreign* intelligence from your Journal, or wish to confine you solely to indigenous literature; but no thinking man, or conscientious

practitioner of physic, can readily bring himself to believe that the *Herculean* practice of our American brethren which has of late occupied the pages of your Journal, can be imitated with impunity in *our island.* The recital of cases lately transmitted to you can answer no good purpose; on the contrary, it is fraught with the greatest danger*. Your Journal is very widely circulated amongst British practitioners, and perused more particularly by the *junior part* of the profession, who anxiously wait the revolving period of its publication, in the hopes of adding something to their stock of knowledge, that might lead them to success in their future practice. What then must be their disappointment, gentlemen, on perusing its contents, where the most prominent feature is the *Brunonian theory* reduced to practice with a vengeance? Numerous instances might be adduced from your latter Journals, some of which (if I recollect right) have already been animadverted upon by some more rational practitioner at home. But too much cannot be said here, in condemnation of American practice.

If they chuse to salivate in phthisis pulmonalis, and at the same time draw blood to the amount of *thirty ounces* daily, for several days together, and deplete in every possible manner, let them enjoy the felicity of seeing their patients escape with life, after putting in force such unwarrantable practice; but why allow them to infuse these *murderous* principles among us?

* The Editors are of opinion, that it answers a good purpose to show what can be done in other climates, as well as the united kingdom; and that their readers possess sufficient discrimination to guard them against the contagion of *rash* practice.

I cannot deny that such is their practice, but I can deny that it would be alike successful in our island. The American mode of living may be different from ours, their stamina more robust; but from such depleting doctors may my countrymen be ever defended!

The patient's account of himself, from whom I have quoted the above *notable* method of practice*, is that in the spring of 1803, he was attacked by phthisis pulmonalis, and, as advised by an eminent physician, immediately began to rub in strong *mercurial ointment*, and to take calomel in as *large doses* as his constitution would bear.

Now, considering this was an *American* constitution, these doses no doubt would astonish a *British* practitioner, but the relator cruelly forbears to inform us whether each dose contained ten, twenty, or thirty grains of calomel, but I think we may venture to place it not less than the first: he was "*previously* reduced by *night* sweats and a plentiful expectoration;" to add to his misery, he was attacked with peripneumony! In forty-eight hours the friction and calomel produced ptyalism, for he very justly remarks, that, under the circumstances above mentioned, there was no *time to be lost!* so that "*thirty ounces* of blood were taken away daily, for several days together," to give the "mercury liberty to operate," a very judicious reason for venesection. So soon as his mouth became affected, "the cough ceased, expectoration abated, and *every* pulmonary symptom began to disappear." In September of the same year, however, he adds, "*Now* I cough none, of *any consequence*, and do not expectorate more than two or three times a

* Vide Medical and Physical Journal for March, 1803, in a Letter to Dr Rush, page 219.

day." Yet this is a case of phthisis pulmonalis *cured* by mercury: and *venesection* should likewise have been added. Yet he is "at a loss to know, whether he should *continue the ptyalism*, or have *recourse to tonics*," although in the very next line he adds, "my *debility* is still *very great*, not being yet able to walk alone." Surprising!

I have only, gentlemen, to apologize to *you* for troubling you with these observations, assuring you they are meant as a friendly hint; the above case *can* require no comment: to state the facts as they are related is sufficient for the discriminating eye of the generality of your readers; I shall therefore only add, that by turning over the pages of your former Journals, *other cases* can be pointed out when the *mode of* treatment was still more preposterous, and unjustifiable upon any solid scientific than that of the preceding, and where the patients were not quite so fortunate, but who were doomed to fall victims to obstinate perseverance in that grand catholicon of transatlantic art, copious depletion. I am, &c.

A PRACTITIONER IN WILTSHIRE.

April 5, 1808. *Med. & Phys. Journ. No.* 111.

Account of a Case of Ossified Uterus. By Arthur Mackie, *M. D. of Lewisham, Great Britain, April 3d,* 1800.

On the 28th of January last I was called on to open the body of Mrs. Ruff, who died the day before, in the 72d year of her age.

She had suffered much from disease, and desired that she might be opened after her death. What I could learn of her

history was as follows: She was born of healthy parents, and was herself strong and healthy till her marriage in her 32d year. She soon after was supposed to become pregnant; but symptoms of dropsy also appearing, she was attended by Drs. Fothergill and Watson, and an accoucheur, who all agreed that it was a case of pregnancy (though her menses continued regularly), accompanied by anasarca; and they prescribed some diuretic medicines. These produced a copious discharge of urine: the dropsical appearances subsided, but the size of the abdomen continued to increase, with more than usual uneasiness, till the supposed time of her delivery.

This anxious period at length passed over, and, to the great alarm of the patient and her family, no labour or delivery took place. From this time she had bad and irregular health; her bulk continued undiminished; she had obstinate constipations; she was frequently attacked with hot, burning, excruciating pains in her belly, that could only be relieved by opium; she became subject to violent gouty paroxysms, which confined her to her bed two or three months in the year, and left her extremities much distorted; and for a long while past, she had been a constant sufferer from the size and increased weight of the abdomen; and, particularly, on turning in bed, when the bulk fell to the lower side, in a lump (as she expressed it), it never failed to give her the most distressing pain and uneasiness. A paralytic stroke, which deprived her of the use of speech, and of one side, preceded her death a few days.

On uncovering the abdomen, it appeared unusually large, hard, and incompressible; the bulk rather exceeding that of a pregnant woman at her full time, and inclining to the left side. In cutting along the linea alba, after dividing the skin, I found the muscles so much ossified, from about three inches above

the umbilicus, that I could not separate them without a very strong knife. The ossification extended as much below the navel, and about three or four inches on each side, having some resemblance to the top of a child's skull. The inside had a smooth, shining polish, apparently from its friction on the uterus, between which and these ossified muscles nothing intervened, the peritonæum being entirely obliterated. On laying aside the separated integuments, the uterus appeared in full view, filling almost the whole cavity of the abdomen, and even pressing against the diaphragm. Its appearance somewhat resembled a very large pellucid bladder distended with hog's lard, and pressed a little flat at the fundus. It was some time before I observed the intestines, which were pushed into a space not exceeding a cube of two inches, in the lower part of the left side of the pelvis, and seemed crowded, shrunk, and empty. The omentum was obliterated. Near them lay above twenty hydatids of different sizes, of a beautiful polished white colour, and several of them as large as pullet's eggs, but without any fluid.

On attempting to cut into the uterus, the knife made no impression, till great force was used, the ossification was so complete: the divided sides appeared of a chalky, brittle, dead white, no vestige of nerves or blood-vessels remaining. The fundus was about a quarter of an inch thick, and perfect bone; the body of the uterus thicker, and not unlike the thickest parts of the occiput. The ossification ended at the cervix, which was of a hardish sebaceous texture, and four or five inches thick. On cutting through this part, about a quart of strong fœtid pus was discharged; and, on making a complete division of the uterus (which was large enough to contain the body of a full-grown fœtus), I found a large detached unformed spungy mass, composed of soft and hard parts; the former resem-

bling moist decayed wood, and easily compressible into a small bulk; the latter somewhat like the ends of the long bones, but no specific part ascertainable; though the whole conveyed to my mind the possibility of its once having been a fœtus.

The uterus with this substance, when taken out of the body, weighed eighteen pounds three quarters, avoirdupois.

The ovaria, Fallopian tubes, and ligamenta rotunda, differed little from their natural size and colour; the liver was small, hard, and of a dark red, and pushed backwards; the gall-bladder appeared as if squeezed by the uterus, part of which was stained with bile. The kidneys were larger than usual, and covered with watery blisters; the vesica urinaria small and empty; the stomach, spleen, and pancreas appeared shrunk, and with the diaphragm pushed upwards.

I was not allowed time to make a more minute examination, or any further remarks; I thought these sufficiently curious to merit being recorded.

Med. & Phys. Journ. vol. III, p. 423.

Observations on the extraction of Opium from the Poppy. In a letter to the Editor from H. Wilkins, *M. D. of Baltimore.*

Last summer I made an experiment on a few poppies that grew in my garden, the result of which I will communicate to you. I have long been convinced that the only desideratum, in opium making, was a method to extract the whole of the gum-resinous juices of the plant. I succeeded so far in this, that I think it now an object worth the attention of gardeners to cultivate the poppy for use as well as beauty. As soon as

the juices had ripened to a rich milky appearance (choosing a clear day), I cut off the head or fructification, and received the overflowing juices on a piece of glass, after which the exudation from the same spot, instead of running down the stem, dried away in the course of two hours, into a little cake of opium. Returning, I cut off with a sharp instrument (a pair of scissars is the best), the above-mentioned little cake, taking with it the least possible quantity of the stem, not more than a line. Instantly after this operation I took off two inches of the stem, and I found, to my great satisfaction, that I had opened the lethean stream afresh, so that I had but to repeat my operations in the same succession and manner as above, till the whole plant was cut down.

I had then two parcels of opium, one on the glass quite pure, and the other consisting of pure opium and clippings of the stem. From these it may be freed by solution in a diluted spirit, and pressure through a hair-cloth.

I am satisfied that any species of poppy will answer. It is a great error, as my experiment will prove, to suppose that such species only as have large capsules will answer; on the contrary, it appears that the more branchy the better. Hereafter we may examine into the relative strength of pure opium prepared from the different species, for we have been very much in the dark about the eastern mode of opium making.

H. Wilkins.

March 24, 1809.

Dr. Sangrado.

The following Biography of a man who has been very undeservedly held up to public derision by Le Sage, in the well-known character of Dr. Sangrado, cannot but be acceptable to those readers, who may not have had an opportunity of perusing it in Hutchinson's Biographia Medica.

Philip Hecquet, a French physician of singular merit and skill, but a strong defender of the use of warm water and of bleeding, for which reason he was ridiculed by Le Sage in his Gil Blas, under the name of Dr. Sangrado, was born at Abbeville, in 1661, and practised first in that city, then at Port Royal, and lastly at Paris. He was not properly *san grado*, for he took the degree of doctor in 1697, and in 1698 had more business than he could attend.

Though attached to the most simple mode of life, he was obliged to keep his carriage, in which he studied with as much attention as in his closet. In 1712, he was appointed dean of the faculty of medicine, and superintended the publication of a sort of dispensary, called "The New Code of Pharmacy," which was published some time afterward. Hecquet was no less zealous in religious matters, than studious in his own profession, and is said never to have prescribed in doubtful cases, without having a previous recourse to prayer. He lived in the most abstemious manner, and in 1727 retired to a convent of Carmelites in Paris, where he continued accessible only to the poor, to whom he was a friend, a comforter, and a father. He died in 1737, at the age of 76. This able physician published several works, none of them devoid of merit. They are the following:

1. " On the Indecency of Men-midwives, and the obligation of women to nurse their own children," 12mo, 1728.

2. " A Treatise on the Dispensations allowed in Lent," two volumes, 12mo, 1705, and 1715.

3. " On Digestion and the Disorders of the Stomach," two volumes, 12mo.

4. " A Treatise on the Plague," 12mo.

5. " Novus Medicinæ Conspectus," two vols. 12mo.

6. " Theological Medicine," two vols. 12mo.

7. " Natural Medicine,"—ditto.

8. " De Purganda Medicina a Curarum Sedibus," 12mo.

9. " Observations on Bleeding in the Foot," 12mo.

10. " The Virtues of common Water," two vols. 12mo.

11. " The Abuse of Purgatives," 12mo.

12. " The Roguery of Medicine," in three parts, 12mo.

13. " The Medicine, Surgery, and Pharmacy of the Poor," three vols. 12mo.

14. " The Natural History of Convulsions."

The life of this illustrious physician was written at large by M. le Fevre de St. Marc, and is not less edifying to christians, than instructive to medical students.

Quacks! Quacks! Quacks!

It is with uncommon satisfaction the Editor opposes to the numerous advertisements of the European continent, the following specimens of domestic growth, which we trust will be found by no means behind the merits of our trans-atlantic brethren. The " stubborn integrity, and perfect scientific knowledge," which so eminently distinguish the learned author

of one of the advertisements, cannot fail of satisfying the judgment of the truth of his remark, that the " notice is given in order to snatch those that are or may be afflicted with that disease* from the hands of quacks or unskilful pretenders."

In hopes of aiding this truly philanthropic spirit, I would propose for perusal to my learned and unlearned readers the 14th and 68th letters of the Citizen of the World, which are replete with wisdom and sound observation.

AMERICAN DISPENSARY.

Doctor Dyott has the honour most respectfully to acquaint the public, that he has established a Medical Dispensary, at his house No. 116, North Second-street, the second door above Race-street, Philadelphia, where they can be supplied with drugs and medicines, warranted genuine, and prepared agreeably to the Pharmacopœia of the royal college of physicians, London.

Persons requiring medical aid, will find in Dr Dyott that *skill* and *stubborn integrity* combined, which *distinguishes his character;* his *perfect scientific knowledge*, united with many years extensive practice in the city of London, enables him to do justice to the afflicted in a manner *very uncommon.*

Mercury, though of itself a noble medicine, the indiscriminate and unqualified use thereof among mankind, has been productive of infinite mischief: our present limits forbid attempting an enumeration of evils, arising from the abuse of mercury; suffice to say, they are numerous, grievous, and destructive to the finest functions of nature. Dr. Dyott, having long witnessed the ravages committed on the human structure, as consequences resulting from a certain disease, through its being neglected or improperly treated, it has claimed and obtained his particular attention. It is a melancholy truth, that the indulgence *in this solitary vice* brings on those deficiencies and debilities which frequently prevent and imbitter the matrimonial state; and imposes by anticipation upon its unhappy victims all the *impurities of old age:* to such persons it must be a consol-

* *Quere:* What disease is here alluded to? It cannot be the one denominated " a certain disease," as I never heard it spoken of before as a " solitary vice." EDITOR.

ing reflection to know that they may now, by a new and infallible mode of treatment, be restored to health, strength, and manly vigour. This public notice is given in order to snatch those that are or may be afflicted with that disease from the hands of quacks or unskilful pretenders; and to direct them where they will receive the most tender treatment, and have a cure performed in a few days, at a very small expence.

Persons having contracted a venereal complaint, are admonished not to tamper with their constitutions, but do *justice to their conscience, and meet every advantage* by making early application to Dr. Dyott, who may be consulted, by patients of either sex, with the greatest delicacy, secrecy, and honour. The apartments are detached for separate consultation, and, in order to afford every convenience and facility to patients, attendance will be given till 10 o'clock in the evening.

From the American Daily Advertiser.

WONDERFUL CURE.

MR. POULSON,

I send for insertion the following extraordinary cure of a cancer. This disease, which has hitherto been accounted incurable, and which, like the small-pox a century since, when it seized on the patient, hurried him into hopeless despair, now sinks before the hands of the physician, and its clouded mists are dispersed by the light from the medical firmament.

Mrs. Mary Schwytzer, who resides in Fourth, near Vine-street, aged about 40 years, was attacked in the face, about two years since, with the cancer, which had devoured one-third of the nose; repeated application to check its ravages were used, but used without success. While she was in this awful situation, a friend informed her of the cures which had been effected by a physician* from Reading, Pennsylvania, and she immediately applied to him for assistance, who, in about six weeks, completely removed the disease, and restored her to perfect health. During the cure, she was not one hour prevented from pursuing her usual business, nor confined at all to the house. She gives the above statement for the benefit of those who, like herself, are labouring without consolation under the scourge of that formidable enemy to the health and happiness of society.

* Dr. Green, Race-street, No. 149.

I do hereby certify, that the above is a true statement, and request the different printers to give it an insertion.

MARY SCHWYTZER,
North Fourth-street, No. 93.

City of Philadelphia, ss.

On the seventeenth day of March, eighteen hundred and nine, personally came and appeared before me John Barker, mayor of the said city, Mrs. Mary Schwytzer, who, on her solemn oath, declares the above statement of facts by her subscribed, are true.

Witness my hand and seal.

JOHN BARKER, Mayor.

The following, on the same subject, is from the Port Folio of October 24, 1801.

I am in the habit of examining, with very eager attention, that "folio of four pages," vulgarly called a gazette or newspaper. For obvious reasons, I generally prefer an English paper to one of our own journals. When I have the good fortune to obtain, from some city correspondent, a file of the True Briton, or the St. James's Chronicle, I do not examine the dates, or, like Murphy's upholsterer, explore the fate of an Indian prince, or the emperor of Morocco; I look for curious advertisements or whimsical paragraphs, and discover much drollery in that department of a public paper, which is supposed devoted to the dry and the dull.

What, for example, can be more facetious than the various advertisements of perfumers, tailors, razor-makers, and quacks? Here, you are informed that L'Eau de Ninon is a perfectly innocent, speedy, and efficacious purifier of the skin. There, you are taught the virtues of Naples dew, and golden water. Mr. Jones tells you that his rheumatic tincture owes its peculiar merit to a vegetable production: and Henry Cundell eulogizes his improved balsam of honey, as though he were the *queen bee* herself. Then who is there who would not live, when the real restorer of life is to be had, for only 10s. and 6d. per bottle? and who would not dislocate a limb, for the privilege of embrocating it with the veritable oil for fractures? How charmingly Dr. Swainson

theorizes upon the disease, scurvy, when he tells you it would be more accurate to call it "atony, debility, want of force or vigour in the whole frame, induced by *the accidents of promiscuous love*, by the *quackery of domestic medicines*, and by the dreadful custom of apothecaries and surgeons applying mercury, antimony, arsenic, &c." What a display of classical learning is made by James Ryan, surgeon, when he informs the wondering student, that "the herb, coltsfoot, was called tussilago, *by the ancients*." Who would scruple to make the frequent journey to Caledonia, or lie every night between two Scots fiddlers, if he can only carry in his pocket Jackson's original ointment, which cures 15,000 persons annually, and was never known to fail The cardiac tincture is sovereign for the attenuation of the fluids, and Sharp's concave razors shave exceeding smooth and pleasant. The cream of Euphrasy

"Will give dim eyes to wander leagues around."

And a saving of one guinea in three may be made by wearing Collyer's silk stockings. Universal philanthropic pills with cure *every disorder*, and a bold dentist will forfeit a thousand pounds, if his patients, after using his "curious compound," have the tooth-ach, or the least foulness of breath.

My limits prevent farther description of the mosaic work of English newspapers. The subject will be again resumed, on a future occasion. More of the sublime and beautiful of English advertising eloquence shall be rehearsed, and merry bills be quoted from those empirics, who, in the words of the sneering bishop Hall,

Give a dose for every disease
In prescripts long, and tedious recipes.

REVIEW.

Observations on the Means of preserving the Health of Soldiers and Sailors; and on the Duties of the Medical Department of the Army and Navy: with Remarks on Hospitals and their Internal Arrangement. By Edward Cutbush, *M. D. of the Navy of the United States.—Philadelphia, Dobson, 8vo. p.* 336, *price* $2.50.

IN a preface of several pages, the author briefly enumerates the various sources of his information, together with the intention of the present work, and what may be expected from its perusal.

In the commencement of the work itself, the author proposes to make "a selection of such regulations as are inculcated by writers who have actually experienced the vicissitudes incidental to a military or naval life," from whom he reasonably supposes more instruction is to be attained, than from the writings of those who have never witnessed the operations of an army or navy; and proposes, to these, to add such observations as service or reflection have suggested to himself.

He then divides the subject into two heads, viz. 1st, the choice of troops, their clothing, and the preservation of their health; 2dly, the preservation of the health of seamen.

The doctor has given us some important observations relative to the choice of troops, in respect to age, constitution, character, and temperament, &c. which are highly important, and well worth the consideration of those on whom the choice of troops depends. The clothing of the troops is next very judiciously adverted to; together with their garrison and winter quarters.

The discipline and exercise of the troops are next considered, as intimately connected with the preservation of their health. In this section many very judicious and pertinent remarks are made, evincing considerable attention on the part of the author to this point. The same may be said of his observations on the subsistence of troops, as it respects their drinks (in which the articles are properly animadverted on) and their aliments.

The means of correcting the influence of climates are judicious, and deserve the particular attention of those to whom this branch of duty more particularly devolves.

The medical practitioner will find much interesting information in that division of the subject which treats "on the opening of a campaign, and the different movements of an army which may influence the health of soldiers;" and we trust that it will not be lightly passed over by those who have the care of so many valuable lives committed to them.

Of the mode of living and duty in camps, although the subject is not familiar to us from experience, yet it appears highly judicious and worthy of attention.

Our author next proceeds to " the consideration of the health of seamen," in which, from an experience of several years, he is more at home; and which, as founded on his own, and the experience of others, we must consider as highly interesting and worthy of notice.

In his observations on the duty of the surgeon-general and other officers of the medical department, the doctor has evinced much attention ; and his remarks will be duly appreciated by those whose situation renders them peculiarly the objects of this division.

In his " remarks on hospitals and their internal arrangements" he gives an interesting account of the origin of hospitals as attached to armies; with a general view of the plan pursued by some of the European nations in their management; and he gives some excellent observations relative to the diet, &c. of these institutions, which are founded on his personal experience of some years, as resident apothecary to the Pennsylvania hospital, which we would suggest, as worthy the attention of the managers of that noble institution. Among other parts deserving of notice, and which we recommend to those who have the necessary power attached to them, is a proposal to remedy the sufferings of sick and wounded, by transportation in common waggons from a camp or field of battle, by means of sacking bottoms peculiarly made, and adapted to be hung by spring hooks to the waggons (see p. 189). The doctor sums up his remarks on this division of his work by enforcing the necessity of strict attention to cleanliness.

The more particular duties of the hospital surgeon and assistants, are next treated of in a concise yet satisfactory manner; and are followed by observations on the naval medical

department, in which the duties of the various officers of the department are enumerated; some remarks are given relative to hospital ships; tables of various kinds, adapted to different purposes, are laid down; and some cursory observations are made on gun-shot wounds, with the necessary preparations previous to an engagement.

Sundry remarks conclude the work, chiefly of importance to those who are entering into the active scenes which the medical department of the army or navy presents to their view; and which might, we apprehend, have been extended with great advantage.

An appendix on the means of dis-infecting the air is given in a translation from the Italian, which is followed by an analysis of mineral waters from Aikin's Dictionary of Chemistry and Mineralogy; and "directions to nurses and orderly men for the preparation of the diet, &c. for the sick." To these are added "gleanings for the use of sea officers and men," containing the best modes of preserving a variety of articles for use, and for making sundry articles of diet or drink.

The author has added the regulations established in the British naval service, from Turnbull's Naval Surgeon; and Dr. Rush's directions for preserving the health of Soldiers.

We have thus given a cursory view of a work, which, independently of its own intrinsic merit, certainly deserves every encouragement, as being the first of the kind published in the United States by a native American; and rendered more valuable as being founded on the experience of several years service in the navy of the United States. We might have enlarged this review by copious extracts from the work, which,

whilst they proved the industry of the author, would have tended to the reputation of the Medical Museum ; but we trust that what has been said will be sufficient to excite those gentlemen for whom the work was principally written to acquaint themselves more fully with its merits, than could be acquired by those partial and insulated extracts, which could be introduced from it into this work.

An Inquiry into the Use of the Omentum: being an Inaugural Dissertation for the degree of doctor of medicine; submitted to the examination of the Rev. John M'Dowel, LL. D. provost, the trustees and medical professors of the University of Pennsylvania, on the 19th day of April, 1809. *By* James Rush, *of Philadelphia.—Philadelphia, T. & G. Palmer,* 1809.

FROM among the few theses which have been published by the graduates in medicine in the University of Pennsylvania, we shall select some extracts from the thesis of Dr. James Rush, in which he has suggested a new theory of the use of the omentum.

" The uses ascribed to the omentum (says our author) have been many and various ; some of those generally received shall be the objects of consideration.

" I. Its office has been supposed to prevent the injuries from the friction of the intestines upon each other, by furnishing an oleaginous fluid to lubricate them.

" It has long been the practice of physiologists, to measure he intentions of nature by their own ingenuity. Hence the

explanations of the pulsations of the heart; of the equilibrium given to the body by the spleen; and of the lubricating quality of the omentum. I object to this opinion, first, *because it is too mechanical.* Although, strictly considered, every natural operation must be mechanical, yet physiology has often suffered from a confusion of the attenuated laws of vital action, with the grosser rules of the arts or mechanism of common life. Secondly, *it is not necessary.* We know that moisture alone, is sufficient to give facility of motion to parts pliant and polished: now the intestines are under these circumstances, with regard to themselves, and the peritoneum. Thirdly, *by its fat, it cannot lubricate the intestines.* The omentum is not a fat-secreting surface, but a fat-containing cavity: its surface affording nothing except that moisture which is natural to all internal membranes, and this I have spoken of as abundantly supplied without the aid of the omentum. Fourthly, the supposition that it is to *prevent friction*, only substitutes, for attrition, a substance less regular and yielding than the peritoneum it is intended to cover.

" II. A second use which has been ascribed to the omentum is, that it is intended, with other abdominal viscera, to prepare blood for the formation of bile. Here, from an accidental, and at the same time an unavoidable circumstance, is deduced a conclusion not warranted by any proof: *accidental*, as the liver was placed in the cavity of the abdomen, and *unavoidable*, as it could receive its blood from the viscera of no other part. If we infer from this situation of the liver, that a change takes place in the blood of the vena portarum, this change must be produced by each particular viscus, or it must be the joint operation of all. It cannot be the former, for each of the several parts secreting a different substance from the blood, must give a different quality of blood to be returned.

If it be the latter, disease in those parts must prevent the formation of the peculiar hepatic blood. Let it not be said, that disease or obstruction of these preparing viscera is often attended or followed by fatal consequences to the system. To this it may be answered, that these parts are vital in other respects than their relation to the circulation of the blood. Moreover, if the bile require this oily secretion, why is the fat thrown out into cells? It ought to pass directly on to the liver through the veins; as this is not its course, it can get there only through the medium of the absorbents. To suppose it first secreted, effused into cavities, and afterwards taken up by lymphatics, is to admit a prolix, where nature has a more simple process.

" This opinion is grounded upon the fact, of fat being found in the omental branches of the vena portarum; and from this has been explained the oily nature of the bile. The inference from this fact is, that the liver is not a secretory organ, but a filter. It is impossible however that *acini* so minute as to prevent the passage of blood, should give admission to so viscid a substance as fat, of such a size as to be the object of examination by the eye. The bile does not derive its oily nature from the fat; we have seen that it cannot pass by filtration, it must undergo the secretory action. Now the very essence of secretion is an alteration of chemical properties, which if the fat suffer, it cannot form the oil of the bile, for, take the most minute portion of its principles away, and you destroy its nature as fat.

" III. A third opinion advocated for the use of this viscus, is that it serves to keep the intestines warm. If this should need a refutation, I would observe: First, it will apply only to hybernating animals. In their living state, nature has prevented

the evil arising from a defect of warmth, by placing these viscera near the greatest source of heat. Secondly, if it were true, the want of such a provision would be felt elsewhere. Why has not the brain its hood, or why is the thorax without its breast-plate?

"This opinion is deduced from the fact, that very fat people feel the cold less sensibly than those who are lean. But this is to be ascribed to the effect it has upon the nerves. Richerand, in speaking of the uses of the fat, says, 'and finally, it covers and surrounds the extremities of the nerves, diminishing their susceptibility, which is always in *an inverse proportion to corpulency*.' Again, 'In fact, persons subject to nervous affections, constantly join an *extreme leanness* to an *excessive sensibility**.'

"Other opinions of the uses of this organ have been proposed. They will be omitted to give place to one, to the support of which the following pages shall be devoted.

"I shall endeavour to prove the omentum to be *an organ for the secretion of fat, furnished with vesiculæ for its reception, in order to supply the body with nourishment, when the resources by the stomach fail.*

"The arguments for the support of this proposition will be be deferred, till the consideration of some circumstances which are connected with this view of the subject.

"That fat nourishes the body in certain states is no new idea: the phenomena of its existence were too obvious not to

* Physiology. Article lxxvi.

lead to that conclusion. As an old doctrine, it has little meaning. Blood is the only medium of nourishment with which we are acquainted; but the fat has not yet become blood. How is this effected? It may be said that it is produced by the lymphatics and their glandular system. This may be answered, by observing, First, whatever change is the effect of their action, it is certain they do not completely convert it into blood; nay, they do not even form a chylous substance, but a more limpid fluid. Secondly, fat has been found in the branches of the vena portarum. A sufficient proof that it has passed, with little or no alteration, the action of the lymphatics: it being immaterial whether it has gotten into the veins by a set of partial vessels, or has passed the circle of the general absorbent system. The change then must be made after it enters the circulation, and nowhere is there a cause adequate to that, except in the lungs or the liver. It cannot be in the lungs, for chyle has been found in the blood after it has passed through them.

" It remains then for the liver to perform the office in question, and this I shall now consider more in detail.

" ' The design of the liver,' says the author, in the essay referred to, ' I believe to be, to receive the blood from every part of the body, in order to subject that part of it which has not been completely animalized, or divested of its chylous properties, to a secretory process, and afterwards to pour the product of this secretion, mixed with the liquor of the pancreas, into the duodenum, to be absorbed or otherwise taken up by the lacteals, and conveyed with the chyle from the stomach into the blood-vessels, in order to be completely converted into red blood, for the purpose of serving the various and important uses for which that fluid is intended in the human body.' It would consume too much time to give, at large, the facts and reasonings used by

the author to support this proposition. I shall give an abstract of them. The same power that *perfects* the chyle, converts the fat to a like matter; and as this is one of the ideas embraced by this essay, its proof may be required.

" The arguments in support of this doctrine are,"—Here the doctor mentions, in a summary way, the arguments in favour of the liver perfecting the chyle, published in the Medical Museum, Vol. III, by his father, Dr. Benjamin Rush. He then proceeds:

" To the above arguments in support of this opinion of the function of the liver, I shall add the following observations. Doctor Pemberton, in his treatise upon the diseases of the viscera*, has taken a view of the glands of the body, as divided into those that secrete a fluid from the blood for the use of the system, and those that secrete a fluid to be discharged from it. The former he has called glands of supply; the latter, glands of waste. He has satisfactorily shewn, that derangements of the glands of waste are not attended by any alteration in the bulk of the body: whereas disease in the glands of supply, in almost every case, produces emaciation. The liver is an organ, the diseases and disorders of which cause a wasting of the flesh. This gives it a place among the glands of supply. I would however infer that it serves this purpose, more than is generally allowed; for if so great a quantity of bile as 24 ounces, which is secreted in 24 hours, be not excrementitious, how considerable must its effect be in nourishing the body! It is known that gall-stones obstructing the biliary ducts produce emaciation. This has been ascribed to the effect of the irritation of pain,

* See ' A Practical Treatise on Various Diseases of the Abdominal Viscera,' by C. R. Pemberton.

upon the system. I would rather suppose it to be owing to the obstruction to the flow of bile, when the stone is in the ductus communis; and when it is in the cystic duct, to the irritation, by continuous sympathy, causing a vitiated secretion of bile.

" With these views of the liver as established premises, I shall proceed to exhibit the proofs of the proposition laid down, on the subject of the intention of the omentum.

" This viscus seems not to be one of those whose use is inscribed upon it so legibly, as not to need the efforts of reason to decypher it. The appearance it exhibits to the eye, is not sufficient to manifest its operations. It is of such inconsiderable importance to the animal system, as not to give by its own derangements any very sensible alteration to the functions of the body, or at any rate it has its office vicariously supplied. From this view we may see why the separation of parts of it, its adhesions, and every other preternatural state of it, have so little effect upon the body. Hence too the difficulty and inefficacy of any experiments, to which it could be subjected. There exist, however, facts enough connected with its ordinary phenomena, as exhibited after disease, to afford just conclusions of its use. To this add its fitness for the office here ascribed to it. For though the sufficiency of a cause be not an undeniable argument for its agency, yet it is essential to proof.

" It has been said before, that *the omentum is an organ for the secretion of fat, furnished with cells for its reception.* This is true, whatever be the intention of the fat when secreted. It is true, because it is seen. I know of no other argument for its proof. Beside the evidence of our senses, proof requires the mediation of axioms. There is no assertion more self-evi-

dent. I might indeed vary its form, but, like the revolutions of a sphere, it would not bring a greater compass into view*.

" The second part of the proposition to be considered is, that *the omentum is intended for the purpose of supplying the body with nourishment when the resources by the stomach fail.* My belief in this arises from the following facts :

" 1st. *From the situation of the omentum*, being in that part of the body where it can afford none of those offices, which the fat in other parts of the system supplies. These are, giving shape to the body, lubricating for the advantages of motion, filling up unavoidable cavities, and serving as the envelope of some viscus or organ. Here is no rugged angle of feature to be smoothed. Here is no eye whose delicateness the contact of bone would injure.

" 2d. *From its little sensibility*, known from the removal of parts of it, and from its circumstances in hernia and wounds. From this I would infer an aptitude for its intention to admit of variation in its size by its distention with fat : a property which, had it not possessed, would have subjected it to the inconveniences of pain.

" 3d. From its presence in so many animals, taken in conjunction with

" 4th. Its absence in others. It would be tedious to enumerate the different animals in which this viscus is found, as well as those in whom it is wanting. The exceptions to it, give the

" * Perhaps we might urge its great vascularity, and its diseases, hydatids and schirrus, in favour of its being a secretory organ.

best idea of its use. For here, as in the forms of mathematical evidence, the substitution is more easily understood. When the omentum is wanting, its place is supplied by a substance which allows scarcely a doubt of its use, and to ascribe to that substance the generally received uses of the omentum, would be absurd. There is only one office, which these adipose substances appear capable of affording, and that is, the nourishment of the body by their fat. This, indeed, is universally allowed. Haller, speaking of the use of the omentum, says: 'Qua tamen animalia omento destituunter, iis adipis massâ pro omento est; avibus nempe aquaticis aliisque et piscibus*.' But without the argument arising from this circumstance, even were this substitute wanting, still its absence would not be an unanswerable objection. The qualities that animals possess are variously, but not partially bestowed. Hence the want of one is supplied by the presence of another similar to it, or equally important. If the omentum should be wanting in some animals, this would not prove that they were denied the advantages that others received from it, or that it was useless in those that had it. It would be explained by these three circumstances: first, that the animals had other peculiarities of structure, as important; secondly, that fat was to them not so necessary; or, thirdly, that the existence of this fat would be inconsistent with some other property more advantageous to the animal.

"5th. I infer the use I have ascribed to this viscus from the state of the omentum in *hybernating animals*. Besides the quantity of fat throughout the cellular membrane generally, and an omentum much distended with it, these animals have two lateral appendices, like the omentum in their structure and

"* Elementa Phys. vol. vi, p. 381.

intention. Haller speaks of this appearance in some of these animals, ‘ ut sint tria quasi omenta*.’ Now, what is the occasion of this great provision? The only circumstance that distinguishes these animals from others, is their remaining for a certain time, without the resources of the stomach for food. Corresponding to this is the state of the omentum. There is no other peculiarity of these animals that requires this difference. There is not more need for lubrication, not more oil required for the bile, nor is it necessary for any other of the imaginary uses ever ascribed to the omentum.

" The views arising from this class of animals are so interesting to the subject, that they will be considered more fully.

" Cuvier, in his observations on this subject, mentions a fact, which, by indicating a use for the omentum, seems to be an objection to that one I wish to establish. ‘ Le grand *épiploon*, suspendu comme un rideau entre les parois musculeuses du bas-ventre, et les circonvolutions des intestins, modère sans doute un peu les froissemens que ceux-ci pourroient éprouver des premières, et sert particulièrement à retenir dans les intestins la chaleur qui tend continuellement à s’échapper vers la circonference. L’histoire des membranes graisseuses dans les animaux qui hibernent, va nous confirmer dans cette dernière opinion. Lorsque l’estomac est plein d’alimens, cet épiploon est raccourci et relevé sur sa face antérieure, de manière à la recouvrir plus complètement qu’avant. Il rend alors plus particulièrement à ce viscère le service que nous venons de lui attribuer à l’égard des intestins. En même temps le sang passant moins facilement dans ces vaisseaux, coule plus abandomment dans ceux de l’estomac, dont les premiers ne sont

" * Haller’s Physiology, vol. vi, p. 381.

que des divisions, et y sépare une plus grande abondance des sucs gastriques*.'

" What seems to be of importance here is, that the omentum is found investing the stomach, whilst it is employed in digestion. A little inquiry will give this an explanation. Observations on animals with the stomach thus enveloped, have been made when the animal was in its living state, well supplied with aliment from the food it was taking, and before the store of fat was laid up for its winter support. Now, that a membrane, thin and vacillating, should, by the constant peristaltic motion of the intestines, be contorted from its natural dependent situation, is not extraordinary, but purely accidental and natural. Before the action of the stomach has ceased to be required, the wants of the animal have called for an accumulation of fat. When the cells of the omentum have become distended, the most economical situation for this membrane is extended and flat. Hence it leaves the stomach, and is spread upon the intestines. But why, according to Cuvier's idea, does this change take place? The bowels are not more active than the stomach; nor is there any other reason why they require more warmth. The last part of this quotation supposes that another advantage is given to the stomach, by this folding of the omentum causing a greater secretion of gastric liquor. This is not the only viscus to which this office of supplying blood to the stomach has been ascribed. The spleen is even yet supposed to be for that purpose. They are both equally untrue. The greater flux *alone* of blood in any viscus, never tends to increase its secretions. This is proved by the phenomena of the circulation and of fevers. Exercise is not, when violent, immediately followed by

" * Leçon xxii, p. 86.

an increase of all the secretions ; nor are inflammations of the liver and kidneys always attended by a preternatural flow of bile or urine. An unusual quantity of blood, in any part, produces disease, or is disease itself. It would be well, if, by secretion, an organ could thus easily rid itself of its irritating cause. Celerity or energy of chemical attractions alone can give rapidity to secretion.

" Some of the species of the hybernating animals are destitute of the omentum. The account given of such animals will sufficiently obviate any objection that might arise from it. Cuvier, in speaking of the hybernating class, says, " Les oiseaux des mœurs analogues, tels que l'hirondelle de marais, plusieurs reptiles que hibernent de même sont depourvus aussi de ces membranes graisseuses ; il est vrai que leur *peritoine* se charge pendant l'hiver *d'une graisse abondante.*"

" 6th. From the defect of fat in the omentum of the fœtus. ' Tenue in fetu, in adulto homine varie obesum*.' This is a fact of great importance. In the fœtal state the supply of nourishment is nearly uniform. The minute arterial connection of the placenta with the uterus, prevents the effects of a hurried or disordered circulation upon the fœtus. Hence there is but little alteration of the volume of blood sent to it, under every irregularity of the mother. With such a constant and unvaried supply, the office of the omentum in the fœtus would have been unnecessary. Soon after birth the fat is deposited, for then the want of it begins to be felt.

" 7th. I infer it, from its distention with fat more particularly than any part of the body, in those disposed to obesity.

" * Elementa Phys. vol. vi, page 367.

Whatever purpose the fat serves, that must be more eminently in view in the omentum, as it contains the greatest quantity. It has been shown that its offices in other parts of the body have no concern here. Its more obvious and important intention of serving as nourishment, alone remains of its uses.

" 8th. From its state when divested of fat; being reduced to a slender contracted membrane, and so much diminished, as to be apparently not fitted for any purpose in the animal economy. The histories of dissections contain many accounts of a partial or total loss of this viscus. Of these I shall mention one, as quoted by Portal, in the case of a patient who had died of pulmonary consumption. ' The other viscera were flaccid, pale, and void of blood. *The omentum was destroyed*, and the body reduced to such a degree of emaciation, as to resemble a very skeleton*.' Here it was not destroyed, as parts of the body often are, by local disease, nor was it absorbed, as parts are when removed from their natural situation. But in a phthisical subject, where the cause of the death was seated in the lungs, its disappearance could have arisen only from the great demand for nourishment, which attends this lingering disease. Not only in this case was the fat absorbed, but its whole substance, as if the membranes, when they had lost their contents, being useless, were themselves taken for food. Perhaps the ludicrous observation made by Mr. Bell, of the impossibility of lymphatics absorbing themselves, might be urged here. I cannot, however, see any more error in it, than in the intussusceptio and sphacelation of an intestine. But, even admitting the impossibility of this, certainly one lymphatic may absorb another. It will be no objection to this to say, that, whilst the

" * Observations sur la Nature et sur le Traitement de la Phthise Pulmonaire, par Antoine Portal. p. 227.

vessel is in a living state, it cannot be subject to the laws of inorganized matter. In reality I know no difference between the laws of living and of lifeless matter; but, according to either idea, the invisible particles, which are taken up by the absorbents, have not the property which common observation makes the characteristic of life, that is, organization or vascularity.

"9th. From the structure of the omentum. It is vascular for the secretion, and cellular for the reception of fat, and is provided with lymphatics for its removal.

"10th. Lastly, I infer that the intention of the omentum is to afford nourishment to the body, from the connection of certain states of this viscus with the diseases of the liver. The fat of the body, without a liver to change it, would have been useless as to its most important property. The diseases of the latter might be supposed to have an influence upon the former, and accordingly a diseased liver has been found, in many instances, to be accompanied by an unnatural state of the omentum. In some cases where, it is true, this viscus should have been deprived of fat, it has been found preternaturally distended with it. But, fortunately for my argument, the liver has, in these cases, been also diseased. I admit, it may be shown from the accounts of dissections, that the liver has been disordered, and yet the fat of the omentum properly absorbed; nay, the membranes which inclose it have been destroyed. This I shall answer by observing, first, that a diseased appearance in the substance of the liver, does not prove a derangement of its secreting organs; and, secondly, admitting this, much more knowledge on the subject is required to determine that these alterations did not take place before the liver became thus diseased. It it is sufficient, then, if I can give one or two instances of this

correspondence in disease, in the two viscera: the exceptions are explained by the two foregoing observations.

"Lieutaud mentions some cases of this kind. I shall relate one of them. In his observation 227, he says, 'Quidam consistensibus ætatis obesus, et jam dudum *asthmaticus;* tandem in ischuriam lethalem incidit. Secto cadavere, deprehenditur omentem, ob majorem adipis copiam; et pondo trigenti librarum. *Hepar erat mole maximum.*'

"In Heister's observations is related the case of a patient who died of extreme emaciation generally, but in whom the omentum was found 'very fat.' He had before death been long troubled with a diarrhœa, which indeed indicated a disease of the liver, and the dissection proved that viscus to be in a morbid state. But what was of much more importance, as it proved the *secretion* of that organ to be altered, was that the patient, during his illness, was troubled with eructations of matter resembling 'bile in being acrid, but destitute of its bitter or acid taste.'

"Here was a case, where, from the wasting diarrhœa that attended, no fat would have been expected to be found in the body; and yet here is an omentum loaded with it. The liver was diseased, and incapable of converting the fat to nourishment. In this state, then, there was no need of absorption, and consequently we find none. It may be said, that, as it had disappeared from the other parts of the body, may not some other cause than the disease of the liver have been the reason why it also was not taken up? This I would answer by observing, first, that the fat of the other parts of the body being less in quantity than in the omentum, would be much sooner absorbed, and this may have taken place before the liver was so far deranged as to be incapable of the office of chylification; or, secondly, if it shows the agency of any other cause, it is that the lymphatics of the omentum and the other parts of the

body have different degrees of power; a fact that would turn to my advantage; for, if the fat in the two situations have different intentions, we should expect to find some of their other circumstances different.

" Mr. Proust, in his Dissections, relates a case that helps to illustrate this question. A patient, who had been much troubled with bilious complaints, was seized with indigestion and vomiting; and these were succeeded by a diarrhœa, of which, after three weeks' continuance, he died. On opening the body, much fat was found everywhere, and the omentum ' entirely occupied by it.' *The liver was of a remarkable size.* Now, if any thing could have given rise to an absorption, it would have been the wasting of a diarrhœa, and the want of nourishment which a diseased stomach must have occasioned. Yet here is a body ' *embonpoint assez*,' to use his own words.

" The pulmonary consumption, more than any other disease, is attended by an emaciation, arising, most probably, from the chronic nature of that complaint. It is certain that the proper and natural supply of materials for the action of any viscus are necessary for its health. Perhaps, then, the diseases of the liver that attend upon consumption, arise from its want of the proper substance for its action. Perhaps, too, the want of that natural supply gives rise to a vitiated secretion, which may be the cause of the wasting diarrhœa that commonly terminates that disease.

" Concerning the disappearance of fat by disease, it deserves to be remarked, that by fasting the action of the lymphatics is increased. If it were not so, fat could not be removed. Now, we know that the want of food by the stomach, almost always causes an absorption of fat*.

" * Perhaps, under this remark, it might not be amiss to notice the practical benefit to be derived from this disposition of the lymphatics to take on a preternatural action during fasting. It teaches us to endeavour, by abstinence, to pro-

" I have thus gone through the several heads of the reasons for my belief on this subject. If any one or two of them appear not conclusive, or even objectionable, let it be remembered that the evidence does not rest upon them alone. If the whole, taken together, should amount to proof, it is sufficient.

" There remain yet some facts, connected with the subject, which, not being referrible to any of the foregoing heads, may properly be considered here.

" It is said that the omentum of herbivorous animals is in general more fat than that of carnivorous. It also said that the same peculiarity distinguishes those whose motions are sluggish, from those whose habits of life are more active. One answer will be sufficient for both these facts. Nature, in the formation of animal bodies, has never made qualities to oppose each other. Fat may have been very necessary for the nourishment of those animals whose wants required physical alertness; yet the existence of fat would have been inconsistent with this last quality. A more important function would hardly have been made subservient to one less necessary. The fatness of herbivorous animals is owing to the abundance of their food, not requiring much exertion to obtain it. The herbivorous are fat, because they require no activity. Carnivorous are lean, because fat would have been an incumbrance.

" Another fact to be referred to this division of the subject, is the fat found in the abdomen of horses. ' Equis a concusso per magnos motus omento adipem non in venas redire, sed in abdominis caveam quasi fusum elabi, et intestinis circumfundi.'

" It has been said before, that the omentum is a secretory

mote the absorption of collections of water in dropsy. That it should not oftener succeed, is owing to the interference of some cause that continues the disease. If the removal of water were the only object to the cure of dropsy, I have no doubt that abstinence would, in almost every case, be effectual. There are many instances on record of the advantage of this remedy

organ. In this case, the fat is secreted in a preternatural quantity, to such a degree, indeed, that the cells of the membrane cannot contain it. Hence they are ruptured, and it is poured into the abdomen. The idea, that there is a greater quantity of fat, is corroborated by the circumstance of its being altered in quality. It is found 'quasi fusum,' which plainly shows its secretion to be changed: fat, in its natural state, never has that form. Why violent exercise should give rise to this preternatural secretion, I cannot pretend to say.

"Some notice has before been taken of the unequal quantities of fat in different parts of the body. Collections of it are often found about the heart, and in the extremities, whilst other parts are much emaciated. As the removal of the fat is performed by the absorbents, it must be influenced by any irregularities that may occur in that system. In addition, then, to the reasons that have already been given for the partial remains of fat, I shall observe that the lymphatic, like every other system in the body, may be partially diseased. Hence a preternatural activity of one part will cause a speedy removal, whilst a torpor in another will prevent any absorption of the fat.

"Another fact, which goes not so much to support the main object of this essay, as to prove the nutrient property of the fat, and its use in the body, is the circumstances of those persons who are under the effects of famine. Dumas, speaking of long-continued abstinence, says, 'It is worthy of remark, that the examples of this kind have principally women for their object.' Now, it is a fact confirmed by anatomists, that fat is proportionally more abundant in children and *females*.

"I have thus, in a cursory manner, considered some of the leading points of this subject. It has been more my wish to establish a new opinion, than to enter minutely into circumstances already ascertained; although, for the purpose of

the former, much of the latter has been necessary. Many facts and opinions have been omitted, which time would not allow, and which the occasion of my writing forbids me to mention. To have considered the subject in all its connections would have been to extend it to too much length. The unity of nature has forbidden a separation of different sciences; much less can a line be drawn through those subdivisions which have been designated by the names of particular sciences.

"Thus far the subject has been considered in relation to theory. The speculative part of every science is its shadow only. We meet it first, because the light is placed behind it. I shall take leave of it by considering a few of the practical applications of the subject.

"The first inference that presents itself, from the doctrine I have delivered, is an argument for copious and protracted blood-letting. 'In an adult man,' says Richerand, 'of moderate rotundity, the adeps constitutes a twentieth part of the weight of the body.' This, upon a moderate computation, would give seven pounds of fat for the supply of the body; a quantity sufficient for its nourishment a considerable time. That it should not sustain it as long as the same quantity of aliment taken by the stomach, should not surprise us, for some of the effects of famine are to be considered as arising, not from the want of additions to the body, but from the derangements which the system suffers through the medium of the stomach. The proper state of this viscus is necessary to the health of the whole body. When the absence of its natural secretion causes disease there, no wonder that the evil of famine is doubled. We should in vain attempt to subdue a pulse by blood-letting, if, at the same time, we should give rich soups to our patients, and yet even a more nutritious substance than this the body is supplied with by its fat. Depletion,

when required, may be continued till that quantity appears to be exhausted, for not till then will it be effectual. Whence, if not from the fat, can be derived the full pulse which attends a chronic fever, when blood has been copiously drawn, and the stomach has long rejected food? Let not, then, the fear of weakness from venesection or abstinence forbid the use of the lancet.

" Secondly, From the preceding facts and reasonings, can be drawn an answer to the common objections to fasting in the cure of inflammatory fevers, or other diseases requiring depletion. I have already spoken of an *advantage* to be derived from the use of this remedy in dropsy. The *evils* that are apprehended from this mode of treatment, are all prevented by the slow, yet equable supply of nourishment which the fat supplies to the body.

" A third observation, more particularly applicable to the function of the liver, is the case of a diseased stomach, when it cannot perform its office in chylopoiesis. That wisdom which made the stomach, foresaw its derangement; and it completed the fabric of the body, by giving fat to supply the place of food, and a liver for its conversion into nourishment.

" Lastly, let us subscribe to the doctrine of final causes. Aliments, according to their nutrient power, have been divided into grades. Sugar, as containing the most nourishment under the least bulk, ranks first in this order. Oil is next to this; and fat, as uniting other properties for the advantage of the body, was the most suitable substance for its purpose. That obesity which to our eyes may sometimes appear disgusting, is perhaps not a partial evil, but a general benefit. The omentum, which has been considered as useless, or supposed to perform very subordinate functions in the system, appears, from the foregoing facts and reasonings, to be a store of aliment for disease and famine."

A Description of the Apparatus by which the Experiment on the Decomposition of Potash by Iron, has been repeated at the Royal Institution, in London.

The apparatus consists of a common gun-barrel curved with one large and one small curvature, and passed through a portable furnace, to which the pipe of a pair of bellows is admitted through an aperture at its side. The curved part of the pipe hangs downwards; to one of its ends an iron tube of about the capacity of two inches, having a ground stopper, is adapted, for containing the potash, which flows out of it, through a very small hole at the lower end. To the other extremity of the bent barrel, a tube of safety is fitted, containing a little mercury or naptha, to prevent communication with the outer air.

In the experiment, iron turnings, put into the barrel so as to fill a part of the lower portion of its curve, are heated to whiteness; the potash is then slowly fused, and flows on the turnings, where it is decomposed; and its base is found condensed near the other extremity of the barrel.

The proportions from which the best results have been obtained, are about $2\frac{1}{2}$ parts of iron turnings to $1\frac{3}{4}$ parts of potash.

In order to the complete success of the experiment, the whole apparatus should be perfectly dry, clean, and impervious to air; the turnings free from oxidation, and the potash very dry; which last is effected by heating nearly to redness. Pure or crystallized potash, in its usual state of dryness, contains water enough to occasion the failure of the experiment. The tube containing the potash should be surrounded with ice, until the iron turnings are white hot; and that part of the barrel where the metal of the potash sublimes, should also be kept cool during the whole process. The barrel should be carefully luted; and it is proper to examine the lute after it has been exposed to a red heat, in order to repair any cracks which the fire may have occasioned.

At the commencement of the decomposition, hydrogen gas is evolved, and continues to come over during the whole of the process. Towards the end of the experiment, a very intense heat should be continued for some minutes, to drive off the last portions of the metal of potash, which adhere to the iron turnings with great obstinacy.

London Athenæum.

Literary Intelligence.

Messrs. Collins and Perkins, of New York, have issued proposals to publish by subscription, "The Anatomy of the Human Body, illustrated by 125 engravings. By John and Charles Bell, surgeons in Edinburgh."

This highly useful and important work is to be printed in 2 vols. 8vo. on a fine vellum paper, from the 4th London edition. The engravings, on the printed pages, are executed on wood, by Dr. Anderson. The price to subscribers is $4 50 per volume, in boards; $5 bound and lettered.

A selection of reviews from English publications accompanies the prospectus, which cannot fail of evincing the high degree of estimation in which this interesting work is held; and, as it is given at one-third the price of the English edition, must insure the sanction of the physicians of America, as a work in the highest degree worthy of a place in every medical library.

Messrs. Hopkins and Earle have in the press, "A Treatise on the Membranes," by Xav. Bichat, member of the societies of medicine, medical and phylomatic, of Paris, and those of Brussels and Lyons. Translated from the French, by George Williamson, member of the medical and surgical faculty of Maryland.

As the only express treatise upon this branch of anatomical knowledge, and by an author so well known to the world as Bichat, we cannot doubt that it will be highly acceptable to the physicians of the United States.

Obituary.

On the 4th of June, in the 39th year of his age, James Woodhouse, M. D. professor of chemistry in the University of Pennsylvania.

Lately, in Great Britain, the celebrated Dr. Beddoes. The life of this gentleman is preparing for the public, by his friend Dr. Edmond Stock, who graduated in this city some years ago.

Dr. Hawes, well known for his active and indefatigable exertions as secretary of the Humane Society of London.

MEDICAL AND PHILOSOPHICAL

REGISTER.

VOL. VI.....NO. III.

FOREIGN AND DOMESTIC.

A Sketch of Mr. Davy's Recent Discoveries of the Chemical Agencies of Electricity, and of the Decomposition and Composition of the Fixed Alkalies, of Ammonia, the Alkaline Earths, &c.

THE preceding historical sketch* of the discoveries in galvanism shows us, that in the first chemical experiments, made with the column of Volta, the appearance of acid and alkaline matter was observed at the opposite electrified metallic surfaces, in water acted on by a current of electricity. These were supposed by Mr. Cruickshank to be the nitrous acid and ammonia, and by M. Desormes, muriatic acid and ammonia; whilst Brugnatelli imagined the formation of a new and peculiar substance, the electric acid. Mr. Davy, as early

* This refers to another article in the same publication.

as 1800, found that nitro-muriatic acid was produced in a tube containing the positive or transmitting wire, and a solution of soda in the opposite tube ; and also ascertained that their appearance proceeded from animal or vegetable matters which were employed to connect the tubes. The traces of fixed alkali found by Mr. Sylvester, as mentioned in the preceding sketch, Mr. Davy suspected to have been derived from some of the materials employed in the manufacture of tobacco pipes, the clay used for which was employed by Mr. Sylvester in his experiment.

By a series of excellently devised experiments, Mr. Davy ascertained that distilled water in agate cups, being connected by amianthus, and exposed to a current of electricity, acid matter appeared to be separated by the transmitting or positive wire, and alkaline by the opposite wire ; and that the acid was derived from some saline matter existing in the agate ; whilst the alkali was yielded by the impurities contained in the distilled water, and carried over in vivid ebullition. In further proof of the fixed alkali not being generated but evolved, some fixed alkali appeared, in almost every intance, where water, even slowly distilled, in vessels of different substances, was the subject of experiment. When tubes of wax were used, a mixture of soda and potash, and a mixture of sulphuric, muriatic, and nitric acids were obtained ; and with resin, the alkaline matter seemed to be principally potash. From Carrara marble, the water became impregnated with fixed alkali and lime ; and from argillaceous schist, serpentine, grauwacké, and glass, soda was obtained. Having in every experiment with purified water found, that the properties of nitrous acid were produced ; that the longer the operation, the greater the quantity that was obtained ; that volatile alkali was also produced, but in very minute portions ; and that the limit to its quantity

was soon determined, he thought it was natural to account for these results from the combination of nascent oxygen and hydrogen respectively, with the nitrogen of the common air, dissolved in the water; and thought it also evident, that water, chemically pure, is decomposed by electricity into gaseous matter alone, into oxygen and hydrogen.

The acid matter collecting on the positive, and the alkaline on the negative side, in all changes in which acid and alkaline matter were present, manifesting a principle of action apparently related to one of the first phænomena observed in the voltaic pile—the decomposition of the muriate of soda attached to the pasteboard, and to many other instances of separation since observed, Mr. Davy instituted a series of experiments with respect to this subject.

Two cups of compact sulphate of lime, containing about 14 grains of pure water each, were connected by fibrous sulphate of lime moistened with pure water, and placed in the circuit of electricity of the voltaic battery; after an hour, a pure and saturated solution of lime was found in the cup containing the negative wire; and a moderately strong solution of sulphuric acid, in the cup containing the positive wire. Similar satisfactory experiments were made with sulphate of strontites, fluate of lime, and sulphate of barytes. To prove the fact that even very minute portions of acid and alkaline matter might be disengaged by this agency from solid combinations, principally consisting of the pure earths, fine grained basalt, which, in 100 parts, contained only $3\frac{1}{2}$ parts of soda, and nearly $\frac{1}{2}$ a part of muriatic acid, with 15 parts of lime, was made the subject of experiment; and yielded, on the positive side, a fluid smelling strongly of oxymuriatic acid; and, on the other, a substance which seemed to be a mixture of lime and soda. Compact

zeolyte yielded soda and lime; lepidolite, potash; and vitreous lava, an alkaline matter which seemed to be a mixture of soda, potash, and lime. With a view of developing a quantity of alkaline matter capable of being weighed, glass was subjected to the electric influence, when soda mixed with a white powder insoluble in acids, the whole weighing $\frac{36}{128}$ of a grain, was obtained, and a nearly corresponding loss of weight was discovered in the glass. Similar results occurred, with greater rapidity of decomposition, and greater distinctness in the phænomena, when compounds soluble in water were acted upon: lixivium of potash, and a solution of sulphuric acid, being soon extricated from a solution of sulphate of potash. The phænomena were analogous when sulphate of soda, nitrate of potash, nitrate of barytes, sulphate of ammonia, phosphate of soda, succinate, oxalate, and benzoate of ammonia, and alum were used. The acids in a certain time collected in the tube which contained the positive wire, and the alkalies and earths in that containing the negative wire. Muriatic salts uniformly gave oxymuriatic acid; compatible mixtures of neutrosaline solutions containing the common mineral acids, yielded their different bases and different acids separated together in a mixed state, without any respect to the orders of affinity; and, when metallic solutions were employed, metallic crystals with oxide were found on the negative, and a great excess of acid on the positive side. Although saturated solutions suffered more rapid decomposition than weak ones, yet the smallest proportion of neutrosaline matter seemed to be acted on with energy; and, in every case where the results were distinct, the separation of the constituent parts was complete from the last portions of the compound.

The experiments of Messrs. Gautherot, Hisinger, and Berzelius, having rendered it probable, that the saline elements

evolved in decompositions by electricity were capable of being transferred from one electrified surface to another, according to their usual order of arrangement, Mr. Davy formed a suit of experiments with the hope of demonstrating this fact. He connected a cup of sulphate of lime with a cup of agate by asbestus, and filling them with pure water, made the platina wire in the cup of sulphate of lime transmit the electricity, which was received by a wire in the agate cup. A strong solution of lime was thus obtained in the agate cup, and sulphuric acid in the other cup. Reversing the order, the sulphuric acid was found in the agate, and the lime in the other cup. Many other trials were made with other saline substances, with analogous results. When compounds of the strong mineral acids with alkaline, or alkaline earthy bases, were introduced into one tube of glass, distilled water connected by amianthus being in the other tube, and both connected by wires of platina in the voltaic arrangement, the base always passed into the water, when it was negative, and the acid, when it was positive. The metals, and the metallic oxides, like the alkalies, passed towards the negative surface and collected round it. Nitrate of silver being on the positive side, and distilled water on the negative; silver appeared on the transmitting amianthus, so as to cover it with a thin metallic film. The time required for these transmissions seemed to be, in some proportion, as the length of the intermediate volume of water. To ascertain whether the contact of the saline solution with a metallic surface was necessary for the decomposition and transfer, Mr. Davy so connected, by amianthus, the purified water in two glass tubes, with a vessel containing muriate of potash, that the level of the water was higher than that of the solution, and the saline matter was at least two-thirds of an inch distant from each of the wires, yet the alkali soon appeared in one tube, and the acid in the other. Endeavouring to mark the progress of the transfer, and the

course of the acid or alkaline matter in these decompositions, by the means of litmus and turmeric, some very singular and unexpected circumstances were discovered. Distilled water, and solution of sulphate of potash, were each connected, by amianthus, with water tinged by litmus, the solution being negatively electrified; and to detect the acid in its passage, slips of turmeric paper were placed above and below the amianthus, directly in the circuit. The first manifestation of the acid was observed, where it was least expected, immediately above the opposite, the positive, surface, where a redness took place, which slowly diffused itself from the positive side to the middle of the vessel; but no redness appeared above the amianthus, nor about it on the negative side; and, though it had been constantly transmitting sulphuric acid, it remained unaffected to the last. The order of the experiment being then changed, the effect was precisely analogous, turmeric was rendered brown, first near the negative wire, and no change took place in the intermediate vessel near the positive wire. A beautiful experiment, with corresponding results, was made, in a similar way, with muriate of soda, the intermediate fluid being solution of sulphate of silver, with turmeric paper on the positive, and litmus paper on the negative side. As soon as the electric circuit was complete, soda appeared in the negative, and oxymuriatic acid in the positive tube; the alternate products being exhibited in the metallic solution by their peculiar precipitates, whilst neither the turmeric transmitting the alkali, nor the litmus transmitting the acid, had their tints in the slightest degree altered.

Acids and alkaline substances having been thus found to be transferred through vegetable colours without affecting them, led to the inquiry whether they would not pass through chemical menstrua having stronger attractions for them; and, for

this purpose, the menstrua of stronger attraction was made the middle link of the conducting chain. Solution of sulphate of potash was placed in contact with the negative point, and pure water in contact with the positive point, a solution of ammonia being made the intermediate link ; when the sulphuric acid was found to pass through the ammonia, and manifest itself about the opposite, positive point. Similar results ensued when potash and soda solutions were the intermediate fluids ; and the acid was exhibited in a certain period, even when transmitted through the most saturated alkaline lixivium : through which muriatic acid from muriate of soda, and nitric acid from nitrate of potash, were, in the same manner, also transmitted. By an appropriate arrangement, the alkaline matter was also transmitted from neutral salts with base of lime, soda, potash, ammonia, or magnesia, through solutions of sulphuric, muriatic, or nitric acid, to the negative surface. Strontites and barytes passed readily through muriatic and nitric acids ; and, *vice versa*, these acids passed with facility through aqueous solutions of barytes and strontites : but when sulphuric acid was employed, the results were very different. With sulphate of potash in the negative, distilled water in the positive, and saturated solution of barytes in the middle part of the circuit, only a minute portion of sulphuric acid appeared even after four days, but much sulphate of barytes had formed in the intermediate vessel ; the solution of barytes having become so weak as barely to tinge litmus, and having acquired a thick film of carbonate of barytes on its surface. With the muriate of barytes made positive, the acid intermediate, and distilled water negative, no barytes appeared in the water in four days, but much oxymuriatic acid was formed in the positive, and much sulphate of barytes deposited in the sulphuric acid. Such of the metallic oxides as were tried passed through acid solutions from the positive to the negative side, but the effect

was much longer in taking place than in the transition of alkaline matter. Green oxide of iron was thus separated from a solution of green sulphate of iron, in about ten hours, having passed through a solution of muriatic acid. These experiments were repeated with numerous variations. Several experiments of transition were also made upon vegetable and animal substances with perfect success; the saline matter exposed in contact with the metal, and that existing in the vegetable or animal substances, both undergoing decomposition and transfer. The leaf-stalk of the polyanthus thus yielded potash and lime; and a piece of muscular flesh of beef was found to yield soda, ammonia, and lime. From experiments by the means of common electricity, Mr. Davy also determined that there can be no doubt that, *the principle of action is the same in common and in voltaic electricity.*

A general expression of the facts relating to these changes and transitions by electricity, Mr. Davy thinks, may be made in common philosophical language, by saying that, *hydrogen*, *the alkaline substances*, *the metals*, *and certain metallic oxides*, *are attracted by negatively electrified metallic surfaces*, *and repelled by positively electrified metallic surfaces; and*, *contrariwise*, *that oxygen and acid substances are attracted by positively electrified metallic surfaces; and that these attractive and repulsive forces are sufficiently energetic to destroy or suspend the usual operation of elective affinity.* Mr. Davy supposes, that the repellent and attractive energies are communicated from one particle to another particle of the same kind, so as to establish a conducting chain in the fluid, and that the locomotion takes place in consequence. In agreement with this beautiful theory, it was found that acid existed in all the alkaline solutions through which the acids had been transmitted, whenever any matter remained at the original source:

in time, the decomposition and transfer would undoubtedly have become complete. In the cases of the separation of the constituents of water, and of solutions of neutral salts forming the whole of the chain, there may, Mr. Davy thinks, be a succession of decompositions and recompositions throughout the fluid. This idea is strengthened by the failure of the attempt to pass barytes through sulphuric acid, and muriatic acid through solution of sulphate of silver; in which, as insoluble compounds are formed, and carried out of the sphere of electrical action, the power of transfer is destroyed. Thus also magnesia, and the metallic oxydes, will pass along moist amianthus, from the positive to the negative surface; but if a vessel of pure water be interposed, they do not reach the negative vessel, but sink to the bottom. In the experiment, in which a small portion of sulphuric acid seemed to pass through weak solutions of strontites and barytes, it is most ingeniously suggested, that it was carried through by a thin stratum of pure water, where the solution had been decomposed at the surface by carbonic acid; since, when the film of carbonate was often removed, and the fluid agitated, no particle of sulphuric acid appeared in the positive part of the chain. It is easy, Mr. Davy says, to explain, from the general phænomena of decomposition and transfer, the mode in which oxygen and hydrogen are separately evolved from water. The oxygen of a portion of water is attracted by the positive surface, at the same time that the other constituent part, the hydrogen, is repelled by it; and the opposite process takes place at the negative surface. In the middle, or neutral point, of the circuit, whether there be a series of decompositions and recompositions, or whether the particles from the extreme points only are active, there must be a new combination of the repelled matter; and the case is analogous to that of two portions of muriate of soda separated by distilled water; muriatic acid is

repelled from the negative side, and soda from the positive side, and muriate of soda is composed in the middle vessel.

Mr. Bennet had shown that many bodies brought into contact, and then separated, exhibited opposite states of electricity. Volta has since distinctly shown it in metallic combinations, and supposed that it also took place with regard to metals and fluids. Mr. Davy discovered, in 1801, that, in electrical combinations by means of alternations of single metallic plates and of different strata of fluids, alkaline solutions always received the electricity from the metal, and the acid always transmitted it to the metal. These principles he thus applies to the explanation of the general phænomena of decomposition and transference above described. *In the simplest case of electrical action, the alkali which receives electricity from the metal would necessarily, on being separated from it, appear positive, whilst the acid, under similar circumstances, would be negative; and these bodies having respectively, with regard to the metals, that which may be called a positive and negative electrical energy, in their repellent and attractive functions, seem to be governed by laws the same as the common laws of electrical attraction and repulsion;*—THE BODY POSSESSING THE POSITIVE ENERGY BEING REPELLED BY POSITIVELY ELECTRIFIED SURFACES, AND ATTRACTED BY NEGATIVELY ELECTRIFIED SURFACES; AND THE BODY POSSESSING THE NEGATIVE ENERGY FOLLOWING THE CONTRARY ORDER. A number of experiments were made with a view of elucidating this idea, and of extending its application, and were found to tend, in all cases, to confirm the analogy. Thus charcoal with water and nitric acid; the same substance, water and soda, being arranged in different electrical combinations, a positive energy was manifested on the side of the alkali, and a negative on that of the acid, when twenty alternations were put

together. Similar phænomena appeared on the employment of a pile of zinc, moistened pasteboard, and quick-lime. The alkaline and acid substances, capable of existing in the dry and solid form, gave by contact exceedingly sensible electricities. Thus oxalic, succinic, benzoic, or boracic acid, perfectly dry, being touched upon an extended surface with a plate of copper insulated by a glass handle, the copper was found positive, the acid negative; and when metallic plates were made to touch dry lime, strontites, or magnesia, the metal became negative; soda affected the metal in the same way; but potash was prevented from yielding a satisfactory result by its powerful attraction for moisture. Sulphuric acid being decomposed by voltaic electricity, the sulphur separated on the negative side; and the phosphorus, from the phosphoric acid, combined in the form of a phosphuret with the negatively electrified metal. That oxygen and hydrogen, agreeable to the general principle, possess, with regard to the metals respectively, the negative and positive energy, was satisfactorily shown by numerous observations on the agency of their compounds.

As the chemical attraction between two bodies seems to be destroyed by giving one of them an electrical state, different from that which it naturally possesses; that is, by bringing it artificially into a state similar to the other; so it may be increased by exalting its natural energy. Although it may be useless, in the present state of our knowledge, to speculate on the remote cause of the electrical energy; or on the reason why different bodies, after being brought into contact, should be differently electrified, its relation to chemical affinity is, however, sufficiently evident. May it not, this justly celebrated philosopher asks, be identical with it, and an essential property of matter? The coated glass-plates of Becearia strongly adhere

to each other, when oppositely charged, and retain their charges on being separated. This fact, Mr. Davy observes, affords a distinct analogy to the subject; *different particles, in combining, must still be supposed to preserve their peculiar states of energy.* This hypothesis, indeed, seems naturally to arise from the facts, and to coincide with the laws of affinity. The general idea affords an easy explanation of the influence of affinity by the masses of the acting substances, as elucidated by the experiments of M. Berthollet. Allowing combination to depend upon the balance of the natural electrical energies of bodies, a MEASURE might even be found of the artificial energies, as to the intensity and quantity, capable of destroying this equilibrium; and such a measure would enable us to make A SCALE OF ELECTRICAL POWERS *corresponding to degrees of affinity.*

As heat and light result from all intense chemical action, so when bodies artificially brought into a high state of opposite electricity are made to restore the equilibrium, heat and light are the common consequences. The effect of heat, in producing combination, is also hereby illustrated, since in a number of cases the electric energies of bodies are exalted by heat, as in glass, tourmalin, and sulphur.

The great tendency of the attraction of the different chemical agents, by the positive and negative surfaces in the voltaic apparatus, seems to be to restore the electrical equilibrium: thus, in a voltaic apparatus with copper, zinc, and solution of muriate of soda, copper being brought into contact with the zinc on both sides, the equilibrium is directly restored, and the circulation of electricity ceases; and oxygen and acids, attracted by the positively electrified zinc, exert similar agencies to the

copper, but, probably, in a slighter degree ; and being capable of combination with the metal, they produce a momentary equilibrium only. The electrical energies of the metals with regard to each other, or the substances dissolved in the water, in the voltaic and other analogous instruments, seem to be the causes that disturb the equilibrium, and the chemical changes the causes that tend to restore the equilibrium ; the phænomena most probably depending on their joint agency. When a pile of zinc, copper, and solution of muriate of soda, is in its condition of electric tension, the communicating plates of copper and zinc are in opposite electrical states ; and water being, as to electricities of such very low intensities, an insulating body, every copper plate produces, by induction, an increase of positive electricity upon the opposite zinc plate, and every zinc plate an increase of negative electricity on the opposite copper plate : the intensity increasing with the number, and the quantity with the extent of the series. When a communication is made between the two extreme points, the opposite electricities tend to annihilate each other ; and if the fluid medium could be a substance incapable of decomposition, the equilibrium, there is every reason to believe, would be restored, and the motion of the electricity cease. But solution of muriate of soda being composed of two series of elements possessing opposite electrical energies, the oxygen and acid are attracted by the zinc, and the hydrogen and the alkali by the copper : the balance of power is, however, only momentary, for solution of zinc is formed, and the hydrogen disengaged. The negative energy of the copper, and the positive energy of the zinc, are consequently again exerted, enfeebled only by the opposing energy of the soda in contact with the copper, and the process of electromotion continues, as long as the chemical changes are capable of being carried on.

In conformation of this theory it may be observed, that the voltaic pile, when the connecting fluid is water free from air, or concentrated sulphuric acid, exhibits no permanent electromotive power, little or no chemical action going on. But water containing air, and still more if holding loosely combined oxygen, produces continued electromotion; and diluted acids, from increase of decomposition, are above all other substances powerful. Neutrosaline solutions are found to lose their energy as their acid arranges itself on the sides of the zinc, and their alkali on the copper: their powers becoming much revived by mixing the fluid well together. As when an electrical discharge is produced, by means of small metallic surfaces, of the voltaic battery, sensible heat is the consequence, it occurred to Mr. Davy, that if the decomposition of the chemical agents was essential to the balance of the opposed electricities, the effect, in a saline solution, of this decomposition, and the accompanying transfers, ought to be connected with an increase of temperature. A beautiful experiment confirmed the conjecture, and proved, with the other facts just mentioned, *that the decomposition of the chemical menstrua was essential to the continued electromotion in the pile.* The same principles will apply to all the varieties of the electrical apparatus, whether containing single or double plates, proving that, *one property, operating under different modifications, is the universal cause of their activity.* That chemical changes are not the primary causes of the phænomena of galvanism may be fairly inferred: electricity is never exhibited in cases of mere chemical decomposition, nor can the electricity exhibited by the apposition of metallic surfaces be referable to chemical alteration.

These general facts and principles will be found to apply to many of the processes of chemistry, both in art and nature. They offer easy methods of separating acid and alkaline mat-

ter, in combination, in minerals, and of analysing animal and vegetable matters. Muscular fibre had thus potash, soda, ammonia, lime, and oxide of iron, evolved from it on the negative side, and the three common mineral acids, and the phosphoric acid on the positive side. From a laurel leaf, green-colouring matter with resin, alkali, and lime, appeared in the negative vessel, and a clear fluid with the smell of peach blossoms, and containing vegetable prussic acid, was found in the positive vessel. Phænomena also evince, that these powers of decomposition act on living vegetable and living animal matter: and as acids and alkalies may be thus separated from, so there is every reason for believing, that, by converse methods, they may be likewise introduced into the animal economy, or made to pass through the animal organs; and the same thing may be supposed of metallic oxides. These ideas, as Mr. Davy justly observes, ought to lead to some new and important investigations in medicine and physiology.

By pursuing these inquiries, electrical decomposition of neutral salts may be found applicable to the economical separations of acids and alkalies; the true elements of bodies may be discovered; alterations of electrical equilibrium in nature, and electric and meteoric appearances, may be easily explained, and tranquil and constant alterations, in which electricity is concerned, may be detected in the interior strata of our globe. Mr. Davy has indeed ascertained, that many mineral formations have been materially influenced, or even occasioned by these agencies of electricity, and conceives that the association of insoluble metallic and earthy compounds, containing acids, may be accounted for; and from successful experiments expects, that the electrical power of transference may serve to explain some of the principal and most mysterious facts in geology. The slow and silent operations of natural electricity,

in every part of the surface of the earth, he thinks, will probably be found more immediately and more importantly connected with the order and economy of nature, than in the case of its evident and powerful concentration in the atmosphere: investigations on this subject, he observes, can hardly fail to enlighten our philosophical systems of the earth; and may possibly place new powers within our reach.

To Mr. Davy we are indebted for the establishing of the following principles—that all bodies that chemically combined are naturally in opposite states; that if brought into the same state, they no longer enter into union; and that if the difference be exalted, they combine with more energy: that oxygen is, in its properties, opposed to all the other bodies in nature; that, wherever it exists in large proportions in compounds, it is always associated with one species of electrical power; and that it is naturally highly *negative*, and therefore combines with all bodies that are highly *positive;* but if made positive by artificial means, it separates from its compounds, and appears in its pure form.

The truly philosophic genius of Mr. Davy is manifested in the happy and appropriate application of the means of investigation, in his correct and comprehensive view of the phænomena presented to him, in the acuteness of discernment, and accuracy of reasoning, which enable him, not only to explain these phænomena, but also to mark those data, and to establish those principles which supply the power of farther extending his discoveries. By the most sedulous and energetic employment of these faculties, he has succeeded in the analysis of bodies which had never been yet decomposed, and in the discovery of the most important and interesting philosophical facts.

Potash, damp at the surface, was placed on an insulated surface, which was connected with the negative wire of the voltaic battery, and the wire from the positive side was placed on the upper surface of the alkali. The potash fused and effervesced, and small globules of a high metallic lustre, like those of quicksilver, appeared; some of which burnt with explosion and bright flame as soon as formed; whilst others were at first merely tarnished, but became finally covered with a white film. In this experiment, as in other decompositions, the combustible base was developed at the negative surface, and oxygen evolved at the positive surface. The white crust formed on the globules was found to be potash, and to have been produced by the oxygen of the atmosphere. The potash thus formed speedily absorbed water, which was again decomposed, the oxygen combining with the globule, and the hydrogen escaping; the process going on until the globule had assumed the form of a saturated solution of potash. When the globules were strongly heated in oxygen, a rapid combustion with a brilliant white flame was produced, and oxygen was absorbed, by which the globules were converted into a white and solid mass, which was found to be potash, and which exceeded in weight the combustible matter consumed. The separation of these principles seems to be the consequence of the combustible base of the fixed alkali being repelled, as other combustible substances, by positively electrified surfaces, and attracted by negatively electrified surfaces, the oxygen following the contrary order; or, the oxygen being naturally possessed of the negative energy and the bases of the positive, do not remain in contact when either of them is brought into an electrical state opposite to its natural one. In the synthesis, on the contrary, the natural energies or attractions come in equilibrium with each other; and when these are in a low state at common temperatures, a slow combination is effected;

but when they are exalted by heat, a rapid union is the result; and, as in other like cases, with the production of fire.

The inflammable base of potash acting almost on every body to which it was exposed, a difficulty arose as to confining it, until it was found, that it was defended, in a considerable degree, by being invested by a thin film of naphtha recently distilled. At 100° Fahr. the base was perfectly fluid, at 50° soft and malleable, and at about the freezing point it became harder and brittle, and of a crystallized texture. Its specific gravity at 62° Fahr. is to that of mercury as 10 to 223, and to that of water nearly as 6 to 10. It is a perfect conductor of electricity and of heat. It is the lightest fluid body known, and even in a solid state, at 40° Fahr. it swims in double distilled naphtha. To be converted into vapour it requires a temperature approaching that of red heat, when it is raised without alteration. Introduced into oxymuriatic acid gas it burns with a bright red light, and forms muriate of potash. Heated in hydrogen, it seems to dissolve in it, the gas, on passing into the air, exploding with alkaline fumes and bright light; but, by cooling, the base is either wholly or principally depcsited. Brought into contact with water at common temperatures, it decomposes it with great violence, instantaneous explosion, and brilliant flame; a solution of pure potash being the result, and a white gradually extending ring of smoke arising, like that from the combustion of phosphuretted hydrogen. Placed upon ice it instantly burns with a bright flame, a hole being made in the ice containing solution of potash. In these experiments heat arises both from decomposition and combination, sufficiently intense to produce the inflammation, and a part of the globule being, in all probability, dissolved by the heated nascent hydrogen, this substance explodes spontaneously, and communicates the effect of combustion to any of the base

which may be yet uncombined. So strong is the attraction of this metal for oxygen, that it decomposes even the small quantities of water contained in alkohol and ether; forming with the oxygen potash, which, not being soluble in these fluids, is precipitated, and hydrogen is disengaged. It inflames and burns on the surface when thrown into solutions of mineral acids. It readily combines with the simple inflammable solids, and with the metals; with phosphorus and sulphur, it forms compounds similar to the metallic phosphurets and sulphurets. It amalgamates with mercury; but the mercury is freed from it as it unites with the oxygen of the air or of water. The fluid amalgam of this substance and mercury dissolved all the metals which were exposed to it; and in this state of union, mercury acts on iron and platina. It unites with gold, silver, or copper, when heated in close vessels; but when the compounds are thrown into water, the water is decomposed, potash is formed, and the metals are separated unaltered. Its action on oils and resins also shows its strong attraction for oxygen. It reduces metallic oxides, and decomposes glass. A globule placed on moistened paper tinged with turmeric, burns and moves rapidly on the paper, leaving behind it a deep reddish-brown trace.

Soda, when treated in the same manner as potash, yielded nearly analogous results: the inflammable base thus separated, became solid on cooling, and appeared to have the colour and lustre of silver. It was exceedingly malleable, much softer than any of the common metallic substances, and was so easily extensible as to be capable of being welded at common temperatures. Its specific gravity is to that of water nearly as 9 to 10, or more accurately as .9348 to 1. It has a much higher point of fusion than the basis of potash; becoming a perfect fluid at about 180°, and fuses readily under boiling naphtha.

The degree of heat at which it is volatile is not ascertained, it remaining fixed at the point of fusion of plate glass. The chemical phænomena produced by the basis of soda, are analogous to those produced by the basis of potash; but with such characteristic differences as might be well expected. From accurate experiments made to determine the proportions of the peculiar bases and oxygen in potash and soda, it appeared that potash consisted of about 6 parts basis and 1 of oxygen; and that soda consisted of 7 basis and 2 oxygen. The opacity, lustre, malleability, conducting powers as to heat and electricity, and their qualities of chemical combination, determining that these newly obtained substances should be called metals, Mr. Davy has named them POTASSIUM and SODIUM.

During the preceding course of inquiry, Mr. Davy suspected that oxygen might exist in the *volatile alkali*, as well as in the fixed alkalies; and that this was the case, he soon became satisfied by a series of most ingenious and well-adapted experiments; it proving to be a triple compound of nitrogen, hydrogen, and oxygen, in which the oxygen could not be estimated at less than 7 or 8 parts in 100. Oxygen then exists in and forms an element of all true alkalis; so that *the principle of acidity* in the French nomenclature, may be likewise called the *principle of alkalescence.* From analogy alone, Mr. Davy also thought it reasonable to expect that the *alkaline earths* were compounds of a similar nature to the fixed alkalis; and some experiments which he made at this period, went far, he thought, to prove that this must be the case, and that even lime, magnesia, glucina, alumina, and silex, might yield their elements to the methods of analysis by electrical attraction and repulsion.

Although potash and soda had never been considered as metallic in their nature, ideas had been entertained, from the earliest periods of chemistry, that the earths might be metallic substances. Whilst engaged in a series of experiments, which had detected a metallic substance in *barytes*, *lime*, *strontites*, and *magnesia*, Mr. Davy was informed by professor Benzelius, that, in conjunction with Dr. Pontin, he had succeeded in decomposing barytes and lime, by negatively electrifying mercury in contact with them; and that in this way he had obtained amalgams of the metals of these earths. These experiments were repeated by Mr. Davy with perfect success, and extending them to strontites and magnesia, he had analogous results. By combining the methods he had before employed with those of MM. Benzelius and Pontin, larger quantities of amalgams were obtained, and these, by a well-adapted apparatus and ingenious manipulations, had their mercury raised from them by distillation. The residuum of the amalgam of *barytes* appeared as a white metal of the colour of silver. It was fixed at all common temperatures, but became fluid at a heat below redness, and did not rise in vapour when heated to redness in a tube of plate glass, but acted violently upon the glass, producing a black mass, which seemed to contain barytes, and a fixed alkaline basis in the first degree of oxygenation. The metal from *strontites* sunk in sulphuric acid, and exhibited the same characters as that from barytes, except in producing strontites by oxygenation. The metal from *lime* could not be examined exposed to air, nor under naphtha. In one experiment in which the quicksilver was distilled to the greatest extent, the tube broke, and, on the air entering, the metal, which had the colour and lustre of silver, instantly took fire, and burnt with an intense white light into quick-lime. The metal from *magnesia* appeared as a solid, having the same whiteness and lustre as the other metals of the earths,

and quickly changed to a white powder, which was magnesia. Although the proportions of oxygen to the bases could not be ascertained, it was found, that whilst burning they absorbed oxygen, gained weight in the process, and were in their highly caustic or unslacked state. The metallic nature of the alkaline earths being thus established, the new substances were named by Mr. Davy, BARIUM, STRONTIUM, CALCIUM, and MAGNIUM.

Anxious to discover the real nature of the other earths, Mr. Davy made an immense number of experiments, with a hope of accomplishing the decomposition of *alumine*, *silex*, *zircon*, and *glucine*; and, from the general tenor of their results, he found great reason to conclude, that these earths, like the alkaline earths, are metallic oxides; the evidences of their decomposition and composition are not, however, of the same strict nature as those which belong to the fixed alkalis and alkaline earths.

Professor Benzelius and Dr. Pontin found that mercury, negatively electrified in the voltaic circuit, in contact with solution of *ammonia*, expanded to four or five times its former dimensions, and became a soft solid—AN AMALGAM OF THE BASIS OF AMMONIA AND MERCURY. An operation, in which hydrogen and nitrogen exhibited metallic properties; or in which a metallic substance was apparently composed from its elements, induced Mr. Davy to examine the circumstances connected with it minutely and extensively.

This amalgam at 70° or 80° is a soft solid, of the consistence of butter; at the freezing temperature it becomes firmer, and a crystallized mass. Its specific gravity is below 3, water being 1. In the air it acquires a crust of ammonia. In water it

produces a quantity of hydrogen, equal to about half its bulk, the water becoming a weak solution of ammonia. In muriatic acid gas, muriate of ammonia is formed, and a small quantity of hydrogen is disengaged. The quantity of basis of ammonia combined in 60 grains of quicksilver does not exceed $\frac{1}{200}$ part of a grain, and to supply oxygen, to this, scarcely $\frac{1}{1000}$ part of a grain of water is required. Professor Benzelius and Dr. Pontin regarded their experiment on ammonia as a strict proof of the idea Mr. Davy had formed of its being an oxide with a binary basis; and, in further proof of it, Mr. Davy found, that when the amalgam was confined in a given portion of air, the air enlarged in volume, and the pure quicksilver re-appeared: ammoniacal gas, equal to about one and a half of the volume of the amalgam being produced, and a quantity of oxygen, equal to one-seventh or one-eighth of the ammonia, disappearing. The more the properties of the amalgam obtained from ammonia, as Mr. Davy observes, are considered, the more extraordinary do they appear. Mercury, by combination with about $\frac{1}{12000}$ part of its weight of new matter, is rendered a solid, yet has its specific gravity diminished from 13.5 to less than 3; all its metallic characters, its colour, lustre, opacity, and conducting powers remaining unimpaired. But on what, Mr. Davy asks, do the metallic properties of AMMONIUM depend? Are hydrogen and nitrogen both metals in the aëriform state, at the usual temperatures of the atmosphere, bodies of the same character, as zinc and quicksilver would be in the heat of ignition? Or are these gases, in their common form, oxides, which become metallized by deoxidation? Or are the simple bodies not metallic in their own nature, but capable of composing a metal in their deoxygenated, and an alkali in their oxygenated state.

Oxygen, he observes, is the only body which can be supposed to be elementary, attracted by the *positive surface* in the electric circuit: and all compound bodies, the nature of which is known, that are attracted by this surface, contain a considerable portion of it. Hydrogen is the only matter attracted by the *negative surface*, which can be considered as acting the opposite part to oxygen. May not, then, the different inflammable bodies, supposed to be simple, contain this as a common element? Should this be proved, still the alkalis, the earths, and the metallic oxides, will belong to the same class of bodies; since, from platina to potassium, and perhaps to ammonium, there is a regular order of gradation as to their physical and chemical properties.

So rapidly have the valuable discoveries of this gentleman succeeded to each other, that, whilst this account was preparing for the press, his opening lecture at the Royal Institution announced that experiments which he had lately made, had given him reason to conclude that sulphur and phosphorus were triple compounds, containing oxygen, hydrogen, and peculiar bases; that plumbago was an alloy of the pure carbonaceous element and iron; that charcoal and the diamond were the same element; the former of these substances containing also a small portion of hydrogen, and the latter a very minute quantity of oxygen.

Employing the new metals, potassium and sodium, as instruments of analytic research, he has, by their agency, already decomposed the boracic and fluoric acids. He has also, by the same powerful agents, produced some extraordinary phænomena with sulphuretted hydrogen, carbonic acid, and carburetted hydrogen gases, these phænomena leading to the most unexpected and important conclusions. When heated in

contact with the gases just enumerated, these metals burn with the greatest brilliancy, although common combustible substances, when introduced into the same gases, in a state of inflammation, are instantly extinguished. During the experiment with the carbonic acid gas, the carbonaceous matter is represented as being precipitated in a most pleasing and beautiful manner.

Mr. Davy had, in the former part of the Bakerian lecture, referred the origin of volcanoes and subterraneous fires to the exposure of combustible metallic bases below the surface, to the oxygen of air and water: " From such an operation," he says, " intense heat must be generated, igneous explosions produced, the surrounding country convulsed, and the lava as a product of combustion poured forth upon the surface."

From recently discovered facts, Mr. Davy is also led to believe, that the renovation of the atmosphere depends rather upon electrical changes, than upon the agency of the vegetable kingdom.

It had been conceived from the experiments of the Swedish chemists, that the metals are probably compounds. This opinion is, however, not adopted by Mr. Davy; he concludes, indeed, from his experiments, that whenever hydrogen or nitrogen is condensed in a compound, free from oxygen, it forms a metallic alloy. He also conceives that the continual addition which is making to the number of the metals favours the idea, that ultimately there may be found only two species of matter on the globe—oxygenous and metallic, gifted with opposite energies, and capable of entering into an infinite variety of combinations.

Even whilst correcting this sheet, the editor learnt, that Mr. Davy had read another paper before the Royal Society, in which experiments are related, showing, that he had succeeded in the decomposition of nitrogen, which appears to be an oxide of hydrogen, containing a greater proportion of oxygen than even exists in water.

Thus are almost daily discoveries presented to us, every one of which are sufficient to give immortality to their author, and to set aside the most generally received opinions. Neither the phlogistic nor antiphlogistic theories of combustion can now explain several phænomena, which appear, however, to admit of easy explanation, on the principle of negative and positive energy. Chemical qualities are seen to coincide always with certain electrical states of bodies. Acids are uniformly negative, alkalies positive, and inflammable substances highly positive; and all are found to lose the peculiar properties and powers of combination, by a change of their electrical states. Chemical qualities are now shown to depend on electrical powers; and it is not impossible that matter of the same kind, possessed of different electrical powers, may exhibit different chemical forms.

Common muriatic acid, Mr. Davy has found to contain at least a third of its weight of water; and has not been able to procure it free from water in an uncombined state. He has, however, obtained combinations of muriatic acid with phosphorus, phosphoric, sulphuric acid, and with phosphorus, free from moisture; and these compounds, even when fluid, though constituted by matters supposed to be intensely acid, do not act on litmus paper, nor dissolve alkalis, and are non-conductors of electricity; but a very small quantity of water developes their energies, renders them conductors, and enables

them to act violently on litmus and alkaline bodies. With these compounds of muriatic acid, potassium detonates violently even at common temperatures ; the muriatic acid being supposed to be decomposed in the experiment.

Thoughts on the Expediency of Disclosing the Processes of Manufactories; being the Substance of two Papers lately read before the Literary and Philosophical Society of Newcastle upon Tyne. By JOHN CLENNELL, *F. S. A. Edinburgh and Perth.*

PART I.

> And what invention's vigorous wing can bind ?
> Ages to come may new inventions find.
>
> MRS. OPIE'S MAID OF CORINTH.

Among the many subjects that ought to have obtained the notice of literary men, there are some upon which, though the want of their cultivation is daily experienced, the attention of science has never yet been efficaciously fixed. My wish, then, is to introduce one of these to the attention of the public, in considering the expediency of a general and faithful display of the processes of manufactories, in such a manner that every chance may be obtained for effecting their highest improvement, with the least possible expence, by the inteference of the operations of science.

This subject appearing to me to be as yet almost unnoticed, I shall therefore take illustrations from every source that I

can; and as my most anxious wish is to effect all the good my power will allow, if I press into its service objects but seldom associated with it, objects, the study of which, in the opinion of many, are incompatible with the views of a manufacturer, I must shelter myself under that motive with those who would blame me for uniting them.

We cannot do better, I presume, than divide our subject into three parts: First, let us discover, if we can, the advantages, or the reverse, that secrecy has effected; secondly, the direct benefit attending disclosure; and thirdly, attempt to answer some objections urged by the advocates of mystery.

For the first division of our subject, we will take a view of the effects of concealment in those countries where it has chiefly prevailed; and as the principle of reserve, though in different directions, is still the same, whether it be in morals, in politics, or in the hands of the ancient priests, miscalled the teachers of mankind, we may be allowed to draw the same inferences from the same premises; and if we find, in the subjects we may produce, lamentable effects, from adopting the language of mystery and the practice of concealment, may it not justly be allowed, that both the one and the other ought to be discarded?

One advantage that may accrue from uniting in our survey the objects of more refined studies is, that they will relieve the dryness of a subject meant principally for the consideration of the manufacturer; we shall endeavour to mingle the pleasant with the useful; and although we may not insure success, yet it may cheer us in passing through the "palpable obscure," the view of the consequences of mysteries, and may help to enlighten the subject,

"As a glittering gem
"Upon an Ethiop's brow."

And, though a manufacturer, I presume, that those who cultivate classical studies, may not feel their favourite object debased, by its association with those of a more humble, it is true, but certainly of a not less useful nature. The first objects, as the eldest in which mystery is concerned, that claim our attention, are the hieroglyphics of Egypt: should these envelope, if I may so use the term, secrets of importance, who is it but laments the want of an index? If, like the volumes found in the ruins of Herculaneum and Pompeii, they contain nothing but what is now well known, may we not at least regret that this information was again to have the labour of discovery?

Of the richness of ancient architecture, what have we left but melancholy vestiges, "subverted shafts, broken entablatures, damaged capitals, mutilated friezes?" where are the plans from which these were erected? If time or war have nearly destroyed the buildings, African and Asiatic jealousy has concealed or annihilated the original designs.

In mechanics, as allied to architecture, by secrecy and mystery in the founders, the method is now lost by which those enormous masses of stone were piled upon each other, as in nearly all the druidical remains. The cement used by the ancients in building, is another instance, which, but for mystery, might not now have been that of unsatisfied inquiry; in vain have hitherto the builders of the present times attempted its discovery. The composition known to the ancients by the name of glesum, possessed properties partaking in some degree of the brilliancy of our glass, and the ductility of a metal. Petronius

Arbiter tells of a person who formed a cup of this substance, that could bear throwing down in a violent manner on the pavement, without any further injury than a bruise, which was beat out again by a hammer. The mode pursued by the ancients in dying the Tyrian purple, the old art of staining glass, the ancient method of working gems, in short, the preservation of all those arts whose extinction is so frequently deplored, would not only have saved an immensity of labour in prosecuting inquiries for their re-discovery, but that, together with the time and expence consumed in such researches, might have had other and more useful directions. In this view, it is much to be regretted, that no earlier intercourse had taken place between the European and Asiatic nations; or rather, that a desire to penetrate into their sciences had not sooner unfolded itself. What shall we think, if the illustrious discoveries of our European philosophers have been anticipated, by more than a thousand years, by the Brahmins of Indostan, as Mr. Maurice more than insinuates in the 7th vol. of his Indian Antiquities*?

And to what is so much destruction of usefulness, labour, expence, and time to be attributed, but the secrets of the compositions being in the possession of prejudiced men, whose fears for the spread of useful knowledge found its equal only in the ancient priesthood?

Formerly, from the jealousy of the coal-proprietors in Scotland, almost every colliery had its own vocabulary, and the workings were concealed with the greatest care. The consequence was, that much valuable fuel was lost, or locked up from the market. The history of banking too, in the same

* Dissertation on the Literature of the Hindoos, pages 570 to 826.

country, affords an instance of the impolicy of reserve and illiberal competition. Since this branch of business has been better understood, the parties concerned find their interest in the plan of honourable and mutual accommodation. Let then the present age, warned by its losses, secure to succeeding generations, by a contrary method, every possible advantage that its discoveries can produce.

History also laments her deficiencies, in the early accounts of nations. These, however, may be attributed to two causes: either there were no records made, or, if any facts were registered, they must have been surrounded with miracle, or recorded in hieroglyphics; in either case nothing remains but a lamentable proof of the insufficiency of such methods. If there really were none who would communicate, is it much better in the class that compose our manufacturers at present? We listen with pleasure to the recital of improvements in science, in morals, and in legislation; but those objects which are the support of thousands, those objects in which industry finds the widest scope, are scarcely yet participating in the otherwise general improvement!

From the lost arts, and the lost information of past times, the transition is easy to those whose secret is at present confined to one nation. " In the East Indies, the natives, by processes very simple, produce dyes, that European manufacturers have in vain endeavoured to imitate. The paints of China cannot be paralleled in Europe, for the sweetness and brilliancy of their colours, all of which there is good reason to believe are extracted from the vegetable kingdom only. The Indians of America, it is also well known, have many beautiful dyes, with which we are unacquainted; and in Africa the negroes, and the natives of the Brasils, have many plants that

furnish inestimable dyes, which are totally unknown to us. Here, then, opens up an immense field for improvement that cannot be exhausted. The natives of Scotland, and other northern parts of Europe, know how to extract beautiful dyes from many plants of no promising appearance. Among lichens and mosses, in particular, the variety of colours that may be obtained, is almost infinite; some of them inimitably beautiful*." The mode of making Russian leather, that also of forming shagreen, said to be manufactured by the Tartars, the various varnishes of the ingenious and industrious people of China, the composition of their porcelain, together with their modes of dyeing, painting, and enamelling it; we might also instance the kindred manufactory at Dresden, and many others, which, from the confinement they suffer, do not improve; besides that, in proportion to their concealment, so is the risk of their loss to the community.

Let us now turn to the second part of our subject, and give instances to prove the direct advantages attending disclosure. In the former part we looked to results from the experience of the selfishness and prejudices of past times. Objects for this divison will be best furnished by facts of the present day: these shall be our only testimonies and support; and, as the great weight of the question hinges here, it will be well to confine ourselves with a jealous eye to them. May we not assign, without fear of contradiction, not merely as one operating cause, but the sole cause of the improvements of agriculture, that it has courted science to contemplate its concerns, that, in almost every county of the united kingdom, well informed and liberal minded farmers are now found, who freely disclose their methods for the inspection or information of the

* Bee, by Dr. Anderson, vol. ix. p. 285.

public; and the consequence is, that not the public only, but themselves, are benefited by such liberality. One of these improvements it may be allowable to mention, without, in any degree, disparaging the rest. The threshing machine, and its various additions and simplifications, are they owing to mystery and concealment, or to knowledge and display? Medicine has, most fortunately for the human race, met with men studious of its improvement and promulgation, from Hippocrates and Galen, to the names of the present time. What a height of enviable cultivation has it obtained from the liberal spirit of communication that has existed so long! To mention its kindred science, chemistry, is alone sufficient to bring to our recollection the unremitted developements that have recently taken place; but this affords also a happy illustration of our first division, as well as the present one. What better example could be given of the inutility of mystery than a survey of the inane labours of the alchymist? and where can we, as it was before noticed, find a subject better adapted to display the great, the almost incalculable consequences of liberal communication, than in its advances? and by those advances a few manufactories have been also materially assisted. Astronomy too is another instance completely illustrative of the beneficial consequences of the principle we are attempting to establish. Whilst mystery and secrecy prevailed, with what was she conversant? An ally of magic, she assumed to discover good or bad fortune from the ascendancies of the benignant or the baneful planets. Connected with palmistry and physiognomy, she pretended to foretel, from the dispositions of the features of the face, and the lines of the hands, the happiness or misery of her deluded followers. Now, to bring the argument to our own times, as well as more directly to the object in view, viz. the advantages to be expected from having manufactories faithfully displayed, we

shall notice, in general terms, the improvements already introduced by chemistry into some of the arts. It is known that bleaching, dyeing, calico-printing, and a small portion of the rest are daily enjoying the benefits of her labours. Engines too, throughout the cotton, woollen, and linen manufactories, have been suggested, carried into effect, and are yet improving: an Arkwright has planned his models, and produced his machinery; Edmund Cartwright has added further improvements; and, from the successful conjunction of science with mechanics, by Bolton and Watt, we would almost be persuaded of the unlimited improvement of steam-engines, and their universal adoption wherever mechanical agency can be employed. Is it secrecy, or manly and unfettered communication, that has produced all this? Is it prejudice or science? If then mystery has once been so universal, and always, and in every instance, hostile to the true interests of man, why should we any longer continue it? If communication has been of such indisputable advantage in all the sciences, and in some manufactories, can it be believed, that towards those not yet benefited by its efforts, it is dangerous to allure its notice? If an enlightened reciprocal communication has done so much already, is it to be imagined that here it must stop, and not extend its usefulness to the whole circle of the objects of manufacturing industry? Error has frequently assumed the mask, and received the honours, of virtue; and the evil of not calling things by their right names is often severely felt. Such are the effects of an absolute prohibition to examine the processes of manufactories, which is exhibited so frequently to the public eye in "No admittance here without leave;" and this prohibition bears the name of prudential caution! Surely it is time to give it a title more expressive of its consequences in society; nay, so confined a view do we take of our own interest, that we yet meet, in common language, with "The mys-

teries of such a business," "The secrets of such a process." In our very printed indentures of apprenticeship, in uncouth and barbarous language, we have sentiments urging secrecy and reserve, congenial with their antiquated phraseology. It ought to be observed, however, here, that the openness for which I plead, does not, by any means, extend to secrets of profit or loss, strictly so called; and if the terms in indentures are always construed to mean no further, I should have no objections to them, other than that their language is almost obsolete; that they are vague and indefinite; and that their explanations rest too much on the will of the master. It is pretended, in another view, that the great motive for concealment is, lest any productive combination of machinery, or method of performing any operation, should be surreptitiously taken from the inventor. But why so? Lest another should reap the advantage of this fortunate process. But by its discovery, and submission to chemical and mechanical observation, some, or a great part of every one of the articles employed, might be dispensed with, by using only the most correct proportions of each, a knowledge of those proportions being obtained by chemical investigation; and not only so, but a great degree of simplification might be effected: thus capital, time, power, or room might be gained, or not misapplied; and would not these things, leading considerations in the mind of the manufacturer, be sufficient to compensate for others enjoying part of his advantages? It is also to be observed, that had scientific men acknowledged the principle of the danger of disclosure, it would have formed a rampart over which improvement could never have risen; it would have proved a *terminus* to every generous wish; a slaughter-house of ingenuity; under its death-devoting influence the sciences, whose present advances we contemplate with thankfulness, and whose increasing improvability gives the benevolent man increasing cause

for a bloodless triumph: those sciences would now, and for ever, present us with hieroglyphics, the energies of nature deified, the vagaries of the alchymist, the whimsical planetary distribution of beneficks, maleficks, triplicities, &c. of the astrologer, or the presumptuous and heart-rending auto-da-fé of the enthusiast: for the enthusiast, the astrologer, the Roman and Egyptian priests, all had their secret rites, and interior recesses, to which there was "no admission without leave;" and, in the latter instances, as they possessed the power, a gunpowder or electrical deity was elicited, to avenge the cause of his priest, insulted by the exercise of curiosity or free inquiry.

An objection is formed also, grounded on political motives, that the developement of the operations of manufactories is only placing objects in the sight of ministers, for the onus of taxation; on this, several observations offer themselves: in the first place, it is very obvious that those politicians who consider taxes as one great motive for exciting industry and ingenuity, will naturally arrange themselves in favour of our principle. It may also be suggested, that a partial, may often be more dangerous than an open and honourable, discovery. Erroneous statement or silence, may not only induce a false ground for taxation, but, from the mystery employed, afford presumptive proofs to the minister of finance that the business is lucrative, and therefore susceptible of a heavy tax. Nor is this, politically considered, the only evil; the manufacturer may lose more in confiscation, or even in time or vexatious interruptions, occasioned by licensed and ignorant inspectors, than he would have done in consequence of his discarding mystery. Again, may we not with confidence assert, that no barrier can eventually stop the progress of information? The links of the golden chain, said to be suspended from the throne of Jupiter, are not more closely united than is discovery to dis-

covery. The chemist and the engineer, like the pioneers of an army, or the first class of settlers in Pennsylvania, have already cleared away great part of the under-growth, the brambles and briars of secrecy, and the iron-oak of mystery will soon be removed so completely, that the place thereof shall know it no more. Even a year or two ago, some arts were enveloped in absolute darkness, which are now before the public. It may be said that these are chiefly those of other countries: attempts are nevertheless now and then making to enrich our periodical publications, with accounts of some of the English; every fresh communication is one step gained; and though it may be retarded frequently by error, yet there may be something of declaration and of fact in each of them, whilst their false statements may be detected and corrected by others in the same track.

It has been suggested, that science displays such innumerable and interesting objects to philosophers, that observations on the manufactories of the country are unattended to by them; whilst those who are particularly concerned in their improvement, are either so much engaged in the regular routine of attendance necessary in their respective concerns, or that not being in the habit of committing their thoughts to paper in the early part of their lives, the awkwardness attending a first attempt deters them. To these I would reply, that although the study of the arts of life may not yield so sublime an enjoyment, as contemplating the grand and awful scenes that astronomy, or chemistry, or any other of the sciences can disclose; yet, that they are worthy a small portion of the attention of the friend of wisdom and of his species, is, I hope, not to be doubted. The engagements of manufacturers to their business do not so entirely engross their time, but some small part of it might be dedicated to a collection of facts in that business, to which,

they say, they so unremittingly attend ; and if want of custom alone prevents their properly arranging them, which is allowing too much if they do indeed apply so intensely, that very business would suggest its own regular course of observations, which, when attended to, particularly for the purpose of either giving or receiving information, their former inattention to pursuits allied to literature would seldom prove a barrier to their attempts ; but the very wish to communicate would secure in most cases a correspondent action ; it would naturally create a habit of attention, or would influence the manufacturer to seek assistance where he conceived himself defective. But the great question occurs here, who will disclose ? To this I reply, gentlemen who have declined business in each, or any branch, or may be about doing so, if principles of liberality are not powerful enough to induce those to do so who are already in it, with the intention of continuing it. But in the multiplicity of manufacturers, we can scarcely suppose, that there will not be found one at least, if not more, of each branch, who are both able and willing to display their respective processes ; and not only so, but to add to their communication the improvements which they may themselves have discovered. Even of this more inaccessible species of information, instances are not wanting of individual disclosure from the most disinterested motives, and that by men, eminent equally for the strength and reach of their understandings as for the liberality and patriotism of their views.

The late Mr. Wedgewood had a meeting every fortnight at his house, of all the master potters in the neighbourhood, where his discoveries were freely described, and their improvements, if they had made any, were as liberally communicated.

About six years ago, "a friendly association of the iron-masters of the counties of York and Derby was instituted, for the purpose of freely discussing the several subjects connected with their important manufactory, and of mutually communicating their various improvements to any individual member, in order to the general benefit. This idea was proposed by Mr. Dawson, of Royd's Hall, the able director of the Low-Moor iron-works*."

To these can now be added a third instance of equal liberality, but with this difference, that whereas the two former received nothing of immediate personal advantage, but what was part, as it were, of a general stock, the following had a slight recompence presented him, though probably the gift was made more to show a sense of their gratitude and of his patriotic sentiments and exertions, than offered as a reward. The following is copied from the Chronicle, a weekly paper, published at Newcastle, by Mrs. Hodgson.

"On the 19th of February, 1807, died William Simpson, Esq. at Lasswade. He was the first who introduced in England, into the paper-manufactory, the improved method of bleaching, by means of the muriatic acid, and he generously communicated the result of his long and expensive experiments to the trade at large, which they acknowledged by making him a present of a handsome piece of plate."

In what a noble point of view do these men, and such as these, appear, particularly when contrasted with those who have carelessly or wilfully allowed their discoveries to perish with them! The one party we view as the benefactors of their spe-

* See Eighth Year's Report of the Literary and Philosophical Society of Newcastle upon Tyne.

cies, arrayed with the genuine feelings of man, and, like the luminary of day, dispensing blessings around them; the other, like the darkness and unfruitfulness of night, reaping where they did not sow, blasting where they can the generous labours of others, and depriving themselves of the exalted pleasure of doing good, forgetful that to bestow benefits, with the same willingness as to receive them, are equally imperious duties.

But supposing further, that these discoveries are made, and in the possession of a few who are ready to publish them: daring, it may be said, would those few be, who would attempt the plan or direct the arrangement; more so, than the person who first ventured to sea on a float. It were indeed necessary for them, neither to fear boisterous power, the blasting frosts of prejudice, nor the sorrowful lamentations of those disappointed men who wish to entail delusion and ignorance on their species, nor the mischief of the combinations of journeymen, than which there is no greater tyrant of the manufacturer. What degree of toil do they fear, who can with stedfast eyes behold, with an intention to destroy, the monsters Combination and Monopoly, more dreadful to the public, than are to mariners the hideous Acroceraunian rocks!

" Illi robur et æs triplex
  Circa pectus erat, qui fragilem truci
Commisit pelago ratem
  Primus, nec timuit præcipitem Africum
Decertantem Aquilonibus,
  Nec tristes Hyadas, nec rabiem Noti,
Quo non arbiter Adriæ
  Major tollere, seu ponere vult freta.
Quem mortis timuit gradum,
  Qui fixis oculis monstra natantia,

Qui vidit mare turgidum, et
Infames scopulos Acroceraunia ?"
HORACE, Book I. Ode III.

It is granted that manufactories, looking at them in the whole, appear, to an unpractised eye, like the stars of heaven; and like them too, the indistinctness of our present field of view confounds us almost at our first approaches: but being separated, like them, into divisions or constellations, we can examine them at leisure, obtain arrangement, and consequently we can examine them with effect. A division of manufactories may be accomplished in something like the following manner, chemical, mechanical, and those which partake of the two. Under the first may be classed the productions of salt, alum, copperas, Prussian blue, &c.; under the second, the cotton, worsted, linen, and silk manufactories may be arranged; and under the third, the smelting of metals, which requires a combination of chemical and mechanical knowledge.

There are two reasons which have been urged, why we have so seldom found manufactories displayed by their owners; the infrequency, and the supposed impropriety, of the union of literature and business: but these have been so ably replied to in the first volume of the Transactions of the Manchester Literary and Philosophical Society, by Mr. Henry, and by Dr. Barnes in the second volume of the same work, that it might be considered presumption in me offering any additional remarks. I shall therefore conclude this part in the words of Dr. Gregory, on the Usefulness of Literature, in the fourth volume of the above work: "I have attempted to arrange my ideas on a subject which I should wish to see taken up by some more able hand, but which appeared to me of too much importance to be utterly neglected."

To be continued.

Circular Letter of the Philadelphia Linnean Society.

Philadelphia, August 16*th*, 1809.

Sir,

It is in conformity with the directions of the Philadelphia Linnean Society, and in their behalf, that we have now the honour of addressing you. The principal object in establishing this society was the promotion of natural history, and particularly that of our own country. In the whole circle of science, there is nothing more pleasing, more interesting, or more beneficial, than the study of nature; and of all the countries in which this study can be pursued, there is certainly none that presents greater advantages than America.

Without doubt, the only practical method of obtaining a history of the natural objects of our country, is to concentrate the observations made in its various parts. We, therefore, beg of you, sir, as a lover of science, that you would communicate to us the result of your observations on natural history, particularly that of your own neighbourhood.

It is not, by any means, our wish to direct or to limit your inquiries. Your remarks on any of the branches of natural history we will always receive with pleasure. One suggestion, however, we take the liberty of making: our society is now forming a cabinet of plants and minerals, the usefulness of which must, in a great measure, depend on its extent and perfection. In order to advance this undertaking, we beg you to communicate specimens of the plants and minerals of your vicinity, accompanied by notes, designating their scientific and vulgar names when they can be obtained, their locality, and,

in regard to plants, their time of flowering. To these necessary notes you will add such other observations concerning them, as you may deem interesting or useful.

Should you, sir, in compliance with these wishes, favour the society by a communication, you will please to direct it to either of the subscribers, and may be assured of a prompt acknowledgment.

We have the honour to be, sir,

Your humble servants,

SAMUEL BETTON, JUN.
SAMUEL COLHOUN,
SAMUEL HAZARD,
Corresponding Committee for the Philadelphia Linnean Society.

Method of Fining Madeira Wine.

" Much of the most excellent of our wine is, when new, darker than the darkest-coloured port or claret; this colour gradually becomes paler, as the wine gets older, till at last it comes to a deep, rich, golden hue. Nothing but age can completely produce this change, though it may be a little accelerated by racking and fining down; but if milk or blood, or any strong fining is used, though it will reduce the colour, and render it much paler, yet the liquor will be impoverished; and, by the attempt to anticipate the appearance of age, the wine may be rendered nearly as sharp and vapid as if originally bad. The only fining which should be ever used, is the white of fresh hen's eggs: the quantity of from four to six is as much as is necessary for a pipe. Their whites are to be beaten up into a

lather, so as to destroy their adhesive quality; then to be well mixed with a gallon of wine, drawn from the cask, and this mixture is then to be put into the wine, which must be be well stirred for a quarter of an hour. In eight or ten days, if the weather is fine, and in twelve or fifteen, if it is variable, the operation of fining will be complete, and the wine should be racked or drawn off. This ought to be done with care, not suffering any of the lees or dregs to come away. These lees or dregs may be afterwards put in a smaller cask, and, when settled, the clear liquor drawn off; and this operation may be repeated till little except mere sediment is left."

[*Extract from a memoir written by an associate of one of the most respectable houses in Madeira.*

Empiricism.

That an Empiric is generally afraid of his own nostrum, is confirmed by many humorous anecdotes that might be brought forward. On one occasion, we are told, a dealer of this description was obliged to send for a physician, through inadvertence in the use of his own specific. The physician, on feeling his pulse, and going through the usual routine of investigation that attends a visit to a patient, expressed his surprize to find his patient so much alarmed at such an apparently trifling indisposition. "Ah! doctor," replied the empiric, "not so very trifling as you suppose; for, alas! to tell you the truth, I have, in a mistake, swallowed some of my own medicine."

The truth of this we may confirm by stating, that within our own knowledge, instead of requiring the assistance of Dr. Bro-

dum, or taking his far-famed medicines, his family generally put themselves under the care of the regular profession for ailments which it is pretended his specifics can infallibly cure. So much for the principles of those concerned in this shameful traffic, the various branches of which are every day expanding, and assuming new shades of imposition.

It would seem that, even in the days of the celebrated author of Hudibras, the public were equally imposed on by the deceptions of quackery, as at the present day; and we have no doubt it will be gratifying to our readers, to have the picture of an empiric of that period delineated by so able a pen.

"The empiric," says this author, "is a medicine-monger, probationer of receipts, and doctor epidemic: he is perpetually putting his medicines upon their trial, and very often finds them guilty of manslaughter, but still they have some trick or other to come off, and avoid burning by the hand of the hangman. He points his trials of skill, and challenges death at so many weapons, that though he is sure to be foiled at every one, he cares not: for if he gets money, he is sure to get off. For it is but posting up diseases for poltroons in all the public places of town, and daring them to meet him again, and his credit stands as fair with the rabble as ever it did. He makes nothing of the pox, and the running of the reins, but will undertake to cure them, and tie one hand behind him, with so much ease and safety, that his patients may surfeit and be drunk as often as they please, and follow their business, that is, whores and him, without any inconvenience to their health and occasions; and he cures with so much secrecy, that they shall never know how it came about. He professes, no cure no pay, as well he may; for, if nature does the work, he is paid for it; if not, he neither wins nor loses; and, like a cun-

ning rook, lays his bet so artfully, that, let the chance be what it will, he either wins or saves. He cheats the rich for their money, and the poor for charity, and if either succeeds, both are pleased: and he passes for a very just and conscientious man; for, as those that pay nothing ought at least to speak well of their entertainment, their testimony makes way for those that are able to pay for both. He finds he has no reputation among those who know him, and fears he is never like to have; and therefore posts up his bills to see if he can thrive better among those that know nothing of him. He keeps his post continually, and will undertake to maintain it against all the plagues of Egypt. He sets up his trade on a pillar, or the corner of a street: these are his warehouses, where all he has is to be seen, and a great deal more, for he that looks further finds nothing at all."

[*Lond. Med. & Surg. Spect.*

On the Practice of Sophisticating Drugs.

Gentlemen—I am of opinion, that some good may arise from exposing the villanous system of sophisticating drugs, which in this great town is carried on to a pitch truly alarming; I am not sure that some harm may not arise from it, as it may have a tendency to render still weaker that confidence in the profession at large, which *this* and many other causes have, I am sorry to say, for a length of time, been gradually undermining. I, not long ago, by chance, stumbled upon a *pharmacopœia officinalis* of one of those pests of society. I shall, at present, only extract two articles, which will serve to give you an idea of the mischievous consequences which such nefarious conduct may necessarily lead to.

1*st. Unguentum Hydrargyri fortius.*

Take of ivory black, one ounce; hog's lard, one pound. Mix.

2*d. Pulvis Ipecacohannæ.*

Take of emetic tartar, one ounce; linseed powder, one pound. Mix.

While the wretched sufferer under the most horrid disease, with which (for good reasons no doubt) Providence has been pleased to afflict mankind, is amusing himself by rubbing in the former, as a specific, the innocent parent is, perhaps, administering the latter to her more innocent offspring; the convulsions and deadly effects of which are too frequently mistaken for the natural consequences of the disease for which it had been administered.

I remain, gentlemen,

YOUR CONSTANT READER.

[*Lond. Med. & Surg. Spect.*

Medical News.

Dr. Williamson, of Baltimore, is engaged in translating, and has nearly ready for the press, " The Elements of Hygiène, or the Influence of Physical and Moral Things upon Man, and on the Means of Preserving Health. By Etienne Tourtelle, professor at the Special School of Medicine of Strasbourg, member of several national and foreign academies, and an associate of the institution of health and salubrity for the prefecture of the sitting guard at Nîmes." In this treatise,

the author has not been content with taking man from his birth, of following him through the different periods of life, of considering him under latitudes the most opposite; of remarking the influence which climate, government, religion, regimen, &c. have, not only upon his organization, but also upon his mental faculties.

There are now in the press, in this city, an edition of the Works of Dr. Sydenham; also of Dr. Cleghorn's Treatise upon the Diseases of Minorca, with notes, intended to render them both more useful to American readers, by Benjamin Rush, M. D. professor of the institutes and practice of medicine, &c. in the University of Pennsylvania.

A third edition of the doctor's Medical Inquiries and Observations is likewise in the press, revised and enlarged by the author. It is expected all these works will appear some time in November next.

Professorship of Chemistry in the University of Pennsylvania.

At a meeting of the trustees of the University of Pennsylvania, on Monday, July 10, 1809, John Redman Coxe, M. D. was appointed professor of chemistry in that university, in place of Dr. James Woodhouse, lately deceased.

MEDICAL AND PHILOSOPHICAL
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Vol. VI.....No. IV.

FOREIGN AND DOMESTIC.

Thoughts on the Expediency of Disclosing the Processes of Manufactories; being the Substance of two Papers lately read before the Literary and Philosophical Society of New-castle upon Tyne. By John Clennell, *F. S. A. Edinburgh and Perth.*

PART II.

Philosophy will then attain to perfection, when the mechanic labourers shall have philosophical heads, or the philosophers shall have mechanical hands.

Bishop Spratt's Hist. of the Royal Society, 4th Edition, page 397.

(Continued from page [129.])

IN the former part we have taken a view, however confined and imperfect, of the consequence of adopting mystery in manufactures and in the arts, elucidated by the influence it formerly had on the sciences; and it was likewise attempted to be proved, from the consequences of the spirit of research, by which these sciences have been enabled to make so rapid a progress, that similar efforts, introduced into the arts and manufactures, would have similar effects: the facts in these views

were meant not only to strengthen or confirm the proposition, but also, reasoning from experience alone, it was supposed, that the charge of being visionary or merely theoretical could scarcely be attached. The improvements in manufactories, which we may flatter ourselves might be adopted, in consequence of disclosure, must be expected, however remotely, from the once slowly progressive, but now highly accelerated, advances which the sciences have experienced in late years; an acceleration which it is most ardently hoped will continually increase, and be bounded only by the existence of man upon this globe!

In ancient times, to conceal these sciences from general diffusion, much more secresy, much more artifice, were practised than are even now used to circumscribe the progressive or scientific improvement of manufactories; it is well known that the initiated were formerly partakers of knowledge under the most tremendous oaths of secrecy; the terrors of confiscation of property, of loss of cast, disgrace of family, and assassination, hedged around the little that was known, and confined it to a chosen few, whose leading principle was not the dissemination of knowledge for public good, but its private cultivation for increasing their powers of tyranny and controul.

Astronomy and chemistry were introduced as affording examples of the destructive effects of ignorance in astrology and alchemy; I could have instanced, in its improvements by science, the progress of navigation; the difficulty is not in mentioning those that are benefited, but in finding one deteriorated by examination or even disclosure: in manufactures too, as I before hinted, who is it but sees and feels the advantages of the introduction of those engines we already have? but how slow would have been their improvement had not communication assisted the engineer!

In the progress of my inquiries, I have been fortunate enough to meet with some more objections to the principle of disclosure, which I shall now notice, and give such replies to, as I presume will be thought sufficient; to these I will add some observations in support of my opinion, and shall think my efforts fortunate, if they are honoured with the attention of the public. I have been told that even the journeymen have secrets in the parts of the business they practise, which the masters, however anxious to do so, cannot discover, and which the journeymen tenaciously keep from them; consequently, that if the masters were convinced of the advantages of contributing towards that general stock of information I propose, or even possessed a clear knowledge of their business, still a mass of little operations, undigested indeed and irregular, yet most useful in each branch, would continue undiscovered. This however is not so much an objection against the propriety of giving information, as it is a statement of the difficulty in procuring it: but allowing its validity as an objection, it might be replied, first, that they will be developed, with or without the consent of the workmen, by that very intimate knowledge of the nature of each article employed, its mode of operation, its connection with, and influence upon, others used in the same fabric, the different effects of hard and soft water, of length of boiling, of the degree of heat continued or lessened, length of maceration, of the effects of slight variations in the dyeing mixtures, or those of the introduction of very common articles to them apparently unconnected with that art, and a variety of other things, in the very minutiæ of the manipulations, in which it is more than probable the whole secrets of the journeymen may consist. All these an improved chemistry, directed to a complete knowledge of the materials used in his business, must open to the master manufacturer; and as a valuable addition to the advantages of more easily acquiring that sort of information, the use of the micro-

scope might be added in examining the filaments, and their various dispositions, in the linen, woollen, silk, and cotton manufactories, in every different department of the work; and indeed at intervals, in its progress through each stage, this instrument is now used for detecting imperfections in linen, muslin, &c. which, we find from St. Fond, was carried by him into France*; with these means well employed, it is perhaps no great boldness to predict, that any object of advantage, in the possession of the journeymen alone, at present, may be discovered, and that without their assistance: but do these men possess the *ne plus ultra* of our present improvements? or will not rather one fact added to another smooth the progress of the inquirer to the discovery of a third, and finally be like the operation of the wedge in mechanics?

In another view, many facts in one business may illustrate many in another: thus, the wauking or milling of cloth, or of Scots bonnets, is only the felting in hat-making on a larger scale; the shrinking up of woollen stockings in washing is effected by the same principle. The joiner, the ship-carpenter, the gardener, and the tree-planter, may afford a few data, valuable to each other. And the clothier, the hatter, and the worsted-stocking weaver, may contribute each a portion of information in his own business to the wool farmer; thus, if the hatters have some ideas on the nature of felting, which they will not discover, the others may be willing to communicate them: but this subject is already made public by Cit. Monge.

Again: this class of society is not free from the operation of bribes; they may be used as a master-key to unlock their tongues, and make them unriddle their mysteries: it is also to be recollected, that many things in their hands, when discover-

* See his Travels in England and Scotland, vol. ii. page 184.

ed, either have been known as well before, or have not been worth the trouble employed in obtaining the knowledge of them; though concealed, as in a higher circle, by cant terms, mysterious looks, and a vain and empty, though sometimes imposing, confidence.

Again: improve the understandings of these men, show them it is their interest to give, that they may receive; to communicate, that they may obtain communications in their turn: but an answer to any objection of this nature is anticipated in an observation of Mr. Boyle, as quoted by Dr. Johnson, in the beginning of the 201st number of the Rambler: "The excellency of manufactories and the facility of labour would be much promoted, if the various expedients and contrivances which lie concealed in private hands, were, by reciprocal communications, made generally known; for there are few operations that are not performed by one or another, with some peculiar advantages, which, though singly of little importance, would, by conjunction and concurrence, open new inlets to knowledge, and give new powers to diligence!"

As another reply, it may be observed, that those men will doubtless teach their children any useful secrets they may possess: if you cannot convince the old people, the chance, from the spread of education and liberality amongst the lower ranks, is more in favour of communication from the young; and we may, without any imputation of visionary extravagance, at least presume, that the chance for their making disclosures, if they possess any thing deserving the title of disclosure, increases, and will increase, every generation.

Labitur et labetur in omne volubilis ævum. HOR.

And thus may education, by giving, through the media of Sunday schools, a bias towards liberality to even the lowest

classes of the community, by that mean alone repay the time, trouble, and expence of subscribers, &c. to such institutions. But do the journeymen really possess secrets worth knowing, and should the above modes fail to insure their discovery, more than two ways yet remain to unlock the treasure: wind into their good opinions, obtain their favour and confidence by gentleness and kindness; few can withstand the operations of beneficence: should they be proof against these means, there is still left the pick-lock of the human heart: apply to their ruling passion, work upon it for the purpose of discovery, and all the obstacles will melt away like snow exposed to a meridian sun; in fine, every method should be applied short of evil "that good may come."

As a reply within the compass of the present time, and what is beyond a doubt sanctioned by experience, it may be urged, that journeymen are always floating, as it were, from one shop to another, and thus, as the winds bear about with them, and scatter the seeds of many plants, into distant soils, so do these men convey the discoveries of more advantageous combinations, or mechanical operations, into other work-shops; the more there are acquainted with such improved methods, the greater probability of their final and complete display. Under the class of replies, which the objection, if it may be called one, that we are at present combating, has suggested, may be placed another, which though at first sight may seem of little weight, yet I confess it appears to me, from circumstances that I have known in different parts of the country, to be of very great influence in spreading information; the quartering of militia regiments: the privates are, in manufacturing counties, taken from the loom, the bench, the plank, and in fact from every operation of the arts; numbers of these obtain leave, or find opportunities, to work at their trades where their regiment is quar-

tered; and in many instances, for very slight considerations, have been known to discover the superior methods of their counties to masters and men indiscriminately in others. Will it be thought empirical in the writer of this, if he takes the liberty of quoting a circumstance in the history of the manufactory he is engaged in, of the introduction and present diffusion of an article in it, which not only gives a perfect illustration of the principle attempted to be established, the beneficial effects of science on the articles employed in manufactures, in consequence of their being submitted to the inquiries and experiments of the philosopher and chemist, but also of the particular reasoning at present under review, that journeymen, by their working in different shops, distribute their information to each, and indeed that they are by no means so tenacious of any improvement they may possess, as is generally represented. The superior effects of the oil of vitriol (sulphuric acid) to the dregs of porter, ale, &c. without such an addition, in working stuff-hats, was discovered in France by one of her chemists: of its peculiar advantage in hat-making we shall see below; its discovery in that country was the mean of obtaining a generally better character for the French hats in foreign markets. Amongst the English prisoners, during what is generally called the Spanish war, there was one who had been a journeyman hatter, who, during his captivity, frequented, whenever he could, the shops of hat-makers which were in the neighbourhood of his prison: his curiosity was awakened by the introduction of a liquid, in addition to a very materially decreased use of dregs, which he had never before seen applied: by degrees and inquiry, he discovered what it was; its great utility consisted in making that part of the rabbit and hare's wool useful, which was thrown away before its adoption; and giving, in fact, an additional value to the whole of the materials employed in making the hats. On his return home, he sold this almost invaluable discovery for a few flaggons of beer, or a very

slight pecuniary recompence, to different shops full of men, all of whom were anxious to obtain the knowledge he had gained, and these communicated it to their fellow-journeymen and their masters, on as easy or even easier terms: and when it is considered that he was of a peculiarly wandering and unsettled disposition, much more so than that class of men generally are, and they are almost proverbially so, it may easily be concluded, that this very great improvement, discovered originally by a French chemist, was soon disseminated round this kingdom.

Another instance of the beneficial interference of science upon the objects of manufactures, will be found in the life of Michael Adanson, lately published. "His researches led him to the discovery of the two genuine Arabic gums, and, after numerous experiments, he succeeded in extracting from the indigenous indigo plants of Senegal, which differ from the American, a sky-blue colour, a valuable discovery, that had escaped the most expert indigo manufacturers, which the French East India company had sent at different times to Senegal*."

The manufacture of sal-ammoniac was first introduced into Edinburgh, and from thence probably into the united kingdom, by a wandering German, known there by the name of "Sooty," whose pretensions to the secret were examined, and afterwards supported, by the late Dr. Hutton, of that city; the work was consequently established in its neighbourhood, and is the largest perhaps in the kingdom. Are we licensed to say that these are the only advantages either taken from neighbouring nations, or imparted by one workman to the country at large? Even in this town or neighbourhood, in the glass-works, the families of the Henzells and the Tyzacks, if not the Titterys, who

*See particularly in the Traveller of Jan. 29, 1807.

from being extinct are of necessity silent, could declare the impotence of such an assertion: in fact, it is not improbable, but every English manufactory has obligations either to France, Flanders, Spain, Germany, and Holland, or some other of its European neighbours, not merely for many of its improvements, but in a great measure for its very origin.

There are many other instances which might be given of the genuine and beneficial effects of carrying our principle into action; but it must be confessd, that the generality of master manufacturers act, as if they were afraid of the fate of sir Thomas Chaloner, who was excommunicated by the then reigning pope, for erecting the first alum-works in England, at Whitby. Something similar to this terror actuates the minds of most of the journeymen hatters, who dare not deviate from the beaten track, for fear of being made "foul;" in other words, excommunicated.

But well would it have been for the manufacturing interest of this country, had that antipathy towards discovery been confined to journeymen! masters of considerable eminence, nay, the whole, with a very few exceptions, are as much wedded to mystery, as those they employ.

On this part of our subject we may conclude with the Rev. Robert Barrows, in the preface to the first volume of the Transactions of the Royal Irish Academy: "How far experimental science may assist the commercial interests of a state, is clearly evinced, by the success of those several manufactures, in the neighbouring countries of England and France; where the hand of the artificer has taken its direction from the philosopher. Every manufacture is in reality but a chemical process, and the

machinery requisite for carrying it on, but the right application of certain propositions in rational mechanics."

Should there be any who are satisfied in the objection to the principle it is the object of the present pages to defend, that it is visionary, one short reply is sufficient: every good yet proposed, every improvement yet suggested, have, at the period of their first promulgation, received that title; but those who are either to be deterred by such a bugbear, or who join in the cry, may themselves have a better claim to receive, and to retain the epithet, the one party for suggesting what a laugh incapacitates them to defend, the other for attempting to foil endeavours for the benefit of our fellow-beings.

But in the language of a very estimable philosopher of the present day, "Laughter is said to be one of the distinctive characters of the human species, and our pleasures are not so many, that we should neglect any innocent source of amusement. No serious evil is to be apprehended from raillery, especially if attended with good nature; Herschell will not be diverted from pursuing his discoveries, because the inhabitants of Laputa give themselves too much anxiety about the sun's good health." Nor sir Joseph Banks from the study of entomology, by the playful Peter Pindar. "Let the man of wit," continues that respectable author, "enjoy his joke, and the man of experiment, his rational toys, provided both unite in cultivating that amiable spirit of philanthropy, on which the happiness of social intercourse so much depends*."

If, however, the scheme is considered as visionary, the author finds that he will not have to bear that accusation alone.

* Nicholson's Journal, Vol. I. 8vo. series, page 150.

Bishop Spratt, in his history of the Royal Society, page 310, of the 4th edition, has the following remarkable expressions, in relating the progress which that society had then made.

" In composing histories after this manner, they resolve to proceed, till they have not only obtained an account of all the great and most substantial trades, but also of all the less works, and private productions, which are confined to some particular soils, or corporations, or families. As this stock shall increase, they purpose to make it of general use, either by continuing printing the most remarkable of them, or by freely exposing them to the view of all that desire such informations; provided, that at the same time they receive some, they will also communicate others: and they have assured grounds of confidence, that when this attempt shall be completed, it will be found to bring innumerable benefits to all practical arts. When all the secrets of manufactures shall be discovered, their materials described, their instruments figured, their products represented, it will soon be determined, how far they themselves may be promoted, and what new consequences may thence be deduced. Hereby we shall see whether all the parts of the most obvious crafts have been brought to perfection; and whether they may not assist each other, more than has been hitherto endeavoured; hereby we shall discern the compass, the power, the changes, the degrees, the ages of them all; and speedily understand, whether their effects have been large enough, and the ways of producing them sufficiently compendious. In short, by this help the worst artificers will be well instructed, by considering the methods and tools of the best: and the greatest inventors will be exceedingly enlightened; because they will have in their view the labours of many men, many places, and many times, wherewith to compare their own. This is the surest and most effectual means to enlarge the inventions: whose nature is

such, that it is apt to increase, not only by men's beholding the works of greater, but of equal, nay, of less wits than themselves."

If then the principle is visionary, with such as this good ecclesiastical visionary would I live, with such would I die.

An objection of nearly the same nature as the last we noticed, is that of an individual "teaching every body his business," and the conclusions, much in the spirit of ridicule, drawn from such: but it ought to be observed, that if every, or even if any, manufactory is unmasked, the more that know them, the better chance for improvement; a variety of new and rational combinations may be suggested, some of which, if not the most, may lead on to incalculable benefit: again, if every person was acquainted, I will add intimately acquainted, with every manufactory, one great source of imposition would cease; and will any one strictly of integrity, ridicule or trammel a plan, that shall do away, even in part, so great an evil?

I have been asked, Do I think it probable that a person who may have purchased, perhaps at a high price, an improved method of some process, would publish the secret, merely from what may be called a motive of liberality? This is not often the case; nor does it appear to bear in the least upon the object recommended; it is condemning the cause of a good system, from the effect of a bad one: the one case is an individual making private property of, and selling to another, who wishes to make it equally as beneficial and equally as sacred, the manner of a certain process which he has had the good fortune to improve: the other, is the public purchasing any improved method of an individual for the public good. But it is confounding the terms to unite in so intimate a manner the ideas of public or general

good, and disclosure: general good, which necessarily includes that of the individual who communicates, would be the result, it must be obvious that it is not the direct motive: complete remuneration to the individual for complete disclosure is the object meant to be illustrated and meant to be enforced; but, as in the cases of Wedgewood, Simpson, and Dawson, mentioned before at pages [126], [127], we see some of the most respectable men publishing their discoveries (originating in persevering ingenuity), and that from a motive of liberality alone: are there no others to be met with? or must the virtuous actions of these men be branded with the charge of imbecility?

It is urged "that other manufacturers in this country would reap the advantages of their more ingenious neighbours; and consequently that the profit of improvements would be snatched from the original discoverers." But barely to mention, that this is already answered above in the quotation from Mr. Boyle, page [141], or that improvements had better never be effected, lest their advantages should extend beyond the first inventor; "the most sacred and unalienable property," it is added, "is certainly that of a man's ingenuity, and in discovering his inventions, the only return, on the principle contended for, appears to be merely fame, if even that: ought not improvements therefore to be the property of the first inventor?" Such is an objection that has very frequently occurred, and it must be allowed is of very considerable weight; particularly so, if the principle in reality condemned a direct remuneration. But besides what might be termed the remote advantage, remuneration would follow immediately and of course; the same principle that induced individuals to discover their improvements, would form societies for purchasing them, as formerly, if not now, in France, for the good of the public; or rewarding them, as in many instances of the present day is the boast of

Britons; in which case they are a property of their inventors, and I only wish to show the impropriety of their being exclusively so after a proper remuneration is received.

Nothing, in the view of the author, can be more sacred than the right each individual has to the profits of his knowledge, ingenuity, or experience; a course therefore that shall unite not only the remote profit of the discoverer of valuable improvements, but also remunerate him immediately, is that which I have endeavoured throughout to recommend; a plan in which right and duty are so intimately united, that philosophical inquirers might still further advance such discoveries, and that individuals might not lose any thing of their just rights.

It might therefore be recommended to government to give a premium, instead of granting patents, for the promulgation of any discovery; or, if patents are easier given than premiums, that the patentee shall be bound to disclose the whole secret at the expiration of the patent.

But, "government may not be able duly to appreciate the value of each discovery, and consequently may reward some insignificant inventions profusely, and some peculiarly beneficial ones with a very trifling remuneration:" this may be obviated in a considerable degree by an association or board of ingenious men, chemists, agriculturists, engineers, and manufacturers, in each county of the united kingdom, through which a communication or correspondence might be established with a similar association in London, consisting of men whose talents and ingenuity are acknowledged; the claims to be examined and adjusted by the board where the discovery is made, and the London association, in conjunction with every other, to give their remarks on its utility, further simplification, and improve-

ment; all these again to assist in the general circulation and publication of the process, and to procure a still further reward to the inventor, in proportion as the utility of the discovery became apparent.

The following might assist in some degree in determining the meaning of the word discovery, in the intercourses of such a connection, as much would depend on its fixed interpretation: each branch or board to enrol all the processes practised at present in England; and whatever is added afterwards, whether by masters or journeymen (for each should be solicited to communicate), ought to be considered as a discovery, and value given for it according to its importance. A fund might be formed by donations, subscriptions, or any other mode of that kind thought most effective, and also from the profit of the publications of the united societies, and the inventions they have patronized, every three, six, or twelve months: thus the inventor would not only receive his reward at once, and without the misery of toiling for it through years, when it might be at best only problematical, but by the reward being instantaneous and sure, it may excite to inventions, which might never otherwise have been accomplished.

An objection, apparently forcible also, but closely allied to the foregoing, has been used to prevent any further display,

> "To blast the fruits of knowledge in the bud,"

"that manufacturers in other countries, where wages may be lower than this, and who even now are using their endeavours to rival us, would then have their most ardent wishes completely fulfilled; and that we should in that instance be supplying a weapon for our own destruction." An argument of the same

nature was used by the opposers of the abolition of the slave-trade: dismiss your slaves, said they, and the trade is open to every country of Europe: so here, dismiss your mysteries, and your rivals profit by it. But if the argument runs so parallel, and as the slave-trade has been proved to be unjust and impolitic, the inference is as regular that the continuance of mystery in manufactories is equally so: but as a case exactly in point with the principle contended for, I would ask, is printing less lucrative, or injured as a trade, by Luckomb's publication of its history and of its rationale?

To reply to these gentlemen on their own ground, what country, except our own, do we benefit by it? Is it Germany? is it France? but France and Germany have their Encyclopædias of arts and manufactories, and have we benefited by them? If we have, shall Great Britain be arraigned before the nations of the world with the charge of ingratitude or selfishness? If we have not, may this not also be the effect of such a work published in England? But this objection, as I before observed, is nothing more than the former one in a more expanded state, and involving as it were a more considerable portion of society; the reply to it therefore prepares in a great degree a refutation to this; in fact, they each of them resolve themselves into monopoly, and are consequently opposed by the same arguments; in addition to which, it may be observed, that as different counties or districts are differently situated, their attentions must of necessity be directed to various speculations: thus manufactures, rather than other objects of gain, will necessarily be cultivated by many; agriculture, in preference to manufactures, by others; and the reduction of ores by a third division; whilst one part of a nation shall derive their business from the export of coals, others will draw theirs from lime. To extend this view from one country, as united within itself by the natural produc-

tions of its various districts, to many, as bound together by their imports and exports; wine or spirits will obtain the almost exclusive attention of one kingdom, others will be the birth-places, as it were, of the more precious metals; here the climate only producing necessaries, there a superfluity of luxuries: thus Nature herself, independent of the arts and their most essential improvements, has laid the foundation of commerce in the exchange of her productions, and that not only in the same kingdom, but has formed a chain that ought to bind every nation of the globe to every other.

> " Oh fortunatos nimium sua si bona norint
> " Populos!"
> " But war, horrid war,
> " Dispeoples half the earth."

Supposing the whole civilized community possessed of a full knowledge of every advance in manufactories, nations of more ingenuity than others would again take the lead, still further advances would be effected through the whole range of time, of place, of circumstance; thus, arguing from what has been, we may safely assert that improvements have no limits, they always reap advantage from competition, and it would perhaps be a most difficult task to say to what country their present state is most indebted. It may not be uninteresting to mention here a curious circumstance which the author learnt from a gentleman of Leicester, that stocking weaving is said to have been invented by the Rev. Mr. Lee, of Cambridge, during the reign of queen Elizabeth, whose patronage he solicited; this being refused him, he went over to Rouen, in France, was countenanced by that government, whose manufactories he thus increased, and made a fortune to himself.

But indeed the objection which we are now combating is one which every manufacturer of every country might use; this

single consideration will justify the charge of its arising solely from the spirit of monopoly, and it appears to be one of its most depraved suggestions. Of one trade with which the writer is acquainted, the English journeymen will not now allow a Frenchman to work with them, nor will the French journeymen associate themselves with one of this country; the reason they each assign is exactly the same, and precisely in the spirit of the objection which we last noticed, lest either should obtain a further knowledge of the improvements of the other: but was the principle of universal disclosure fully acted upon, other countries would give us in their turn their manufactories, and we should also have the advantage of their ingenuity in assisting ours in a more rapid progress of improvement: and have we, seriously speaking, so far advanced our discoveries, that the knowledge of the whole world besides is of no importance to us?

But to descend from generals, and to produce an instance amongst many, and that no subordinate one, that we may have the opposing argument in its full strength: the cotton manufactory, I believe, is that of the improvements of which we are most jealous: but if for the injury or loss, shall we say, of one, and that no very healthy process, we gain many, or even a few, as before hinted, and these no way injurious, and perhaps equally as profitable, the result is certainly advantageous: to the maiming or probable loss of this, we may oppose the production of Spanish wool in our own country; and perhaps each nation of Europe has even now its favourite manufacture which might visit this kingdom, not driven by sanguinary persecution for religious or even political principles, but allured hither by the gentle and benign operations of knowledge and more liberal encouragement: even now we may discover the dawning of such a progress from the impotence of legal restraint; we have

seen the Spaniard has not been able to monopolize his breed of fine woolled sheep; the French and Italians have received encroachments on their method of rearing silk-worms, and manufacturing the produce of their labours; nor have our prohibitory laws completely frustrated the attempts to transplant machines, for the cotton and woollen manufactories, into foreign countries; what numbers have been shipped, under the concealment of night, in defiance of the risk of detection, of forfeiture, and of penalties! We may in this part also illustrate national by municipal jealousy: we are told that the first machines were obliged to be taken out of Manchester by night for Perth, and are now spreading over many parts of Scotland; yet no decrease of business is felt by the Manchester manufacturers from this circumstance. And indeed how much more cheering to humanity, and agreeable to the singleness and simplicity of virtue would it be, if the improvements of one nation, instead of being introduced into another by bribes or diplomatic intrigue, had been patronized and diffused under the mentally healthy auspices of general improvement, having for its foundation kindness to the whole human race!

A few observations on the consequence of the general adoption of the principle, and I have done.

The impertinence of journeymen, when they find that they are in any degree the superiors of their masters in a knowledge of their business (and, from this, the pliableness of many masters to their whims), would be quickly done away; their laws and combinations would be effectually broken through; the progress of this most desirable object would eventually be more auspicious and more speedily and completely effected, in consequence of the general diffusion of knowledge involving also the progress of that for which I plead, and by taking an increased

number of apprentices, whether from the different parishes or otherwise, rather than by parliamentary interference; though the last act of our legislative body against combinations is certainly a great bulwark to the masters, and, so long as the men can have legal redress, where necessary, for legal application, they are by no means sufferers.

On this head one great support of combinations, and which it might be humbly recommended to the legislature to abolish by every mean in its power, is a practice pursued in many trades, of journeymen, when "on tramp," being taken "upon turn;" that is, when they are wandering from place to place, most probably dismissed from their last masters for their own immorality or depraved behaviour, the journeymen in the same trades at the towns at which they may call, are obliged by their bye-laws to support them, to drink with them, and, if they do not find masters to employ them, to make a subscription among themselves, to set "the strangers" on their road; this by the bye is generally spent ere they part: the effect is that many journeymen wish for the coming of a stranger, as an apology for their ramble: thus, in such instances, and in some trades, drunkenness, and every species of vice, are the consequences of "having a man upon turn;" but was the practice abolished by the interference of government, the effects on the trades where it is practised would be beneficial to an extensive amount.

It has been said, that the man who made two blades of corn grow where one only grew before, deserved the thanks of his country; and are no obligations due to him, who, by inquiry, perseverance, and communication, enlarges the usefulness of those articles already employed in manufactories; who brings

new materials into the market, and forces new subjects to pay their tribute of utility to the arts, and to mankind?

It is obvious that the Royal Society of London, that for the Improvement of Arts, Manufactures, and Commerce, and the Royal Institution, all look to the regular publication of manufactories as one source of their materials, and they have in one degree or another pressed this consideration on the minds of their subscribers. It is, notwithstanding this, curious to observe, that none of them have entered at large into the reasons for such a wish, nor have they in any peculiar manner attended to the objections against such proceedings: it is also singular, that nothing of the kind has as yet appeared amongst the productions of the Literary and Philosophical Society of Manchester. It has been the desire of the author of this to fill up the chasm occasioned by the above oversight. If his success has been equal to the newness of his subject, it will constitute a feeling which he will be proud to possess: if it has not, though he is himself a manufacturer, though his leisure is devoted to literary pursuits, two leading features in the formation of that character who may best defend the principle of general developement, he has yet to lament that he is not the man who is to effect so extensive a reform: his view in this light, if humiliating, is not unpleasing; he can even in that event attend to, and enjoy, the success of others.

One recommendation propositions of this nature indubitably possess: if objects are interesting in proportion to their magnitude, and if that importance is to be valued according to their influence on the interests of society, this under our consideration at present must insure attention, as it embraces the very means of existence of by far the greatest part of the population of the united kingdom!

To sum up what has been said in as few words as possible: manufacturers may be divided into three great classes; those who have no motive but mere indolence to prevent their communications; those possessing no secrets peculiarly profitable, but who assume a mysterious manner perhaps to conceal their ignorance of the business they are concerned in; and those who have processes which, whether from patent-rights, or other apparently strong motives, they are desirous of concealing. The first class——but I pass on to the second; for these it may be sufficient to reply to the objections generally urged against disclosure, which I have attempted to do in the foregoing considerations; and if any truth exists in what I have there stated, it will be readily granted me, that mystery, as it is the bane of private, so is it also the destruction of general improvement: and for the third class, a chamber or board of commerce, as suggested at page [150], might be established, to record and reward their labours.

To the real patriot, the friend of the prosperity of his country, the improvements adopted after a given number of years were elapsed, would form an interesting object of contemplation: each disclosure would be like a pharos or beacon, to direct other inquirers, like a terminus or land-mark, to show how far the former limits extended: man would not then be humiliated by retracing the steps of discovery, but improvement would proceed in an uninterrupted course, like those majestic rivers of America, which, receiving their supplies from innumerable lakes and mountains, increase with their progress, until they pour their full tribute into the Atlantic!

Method of preserving Fruit without Sugar, for House Use or Sea Stores. By Mr. THOMAS SADDINGTON*.

The expence of sugar is frequently urged as a reason for not preserving our English fruits, and to this may be added the uncertainty of success from the strong fermentable quality of many fruits. They may indeed be preserved for a length of time without sugar, by baking or boiling, and then closely stopping them up; but if the cork becomes dry, the atmospheric air exchanges place with what is impregnated by the fruit, which then soon becomes mouldy: but fruits may be preserved in good condition, by the following process, for two years, or even it is probable for a longer period, even in hot climates, as some that were done in 1806 have been exposed in an upper room to the sun during the whole of the summer without injury.

The fruit being clean picked, and not too ripe, is to be put into wine or porter bottles, as they are cheaper than what are called gooseberry bottles, and can be procured in places where the latter cannot be had. The bottles must be filled as full as they can be packed, and corks being stuck lightly into them, they are to be placed upright in a kettle of water, and heated gradually to about 160 or 170° Fahr.; that is to say, until the water feels very hot to the finger, but does not scald. This degree of heat is to be kept up for half an hour, and then the bottles being taken out one by one, they are to be filled up to within an inch of the cork with boiling water, the cork fitted very close and tight, and the bottle laid on its side, that the cork may be kept moist. To prevent fermentation and mould,

* Trans. Soc. Arts, vol. XXVI.

the bottles are to be turned once or twice a week for the first month or two, and one or twice a month afterwards.

When applied to use, some of the liquor first poured off serves to put into the pies, &c. instead of water, and the remainder being boiled up with a little sugar will make a rich and agreeable syrup.

The fruit ought not to be cracked by the heat; some trials were made by keeping the bottles in a heat of 190° for three quarters of an hour, but the fruit was reduced nearly to a pulp.

Although wine or porter bottles have such narrow necks, the fruit may be picked out with a bent wire, or iron skewer.

Samples of apricots, gooseberries, currants, raspberries, cherries, plums, Orleans plums, egg plums, damsons, Siberian crabs, green gages, and rheubarb, have been presented to the society.

In the summer of 1807, it cost about 1*l.* 9*s.* exclusive of the charge of the bottles and corks, to preserve 95 bottles of fruit; which were worth in the winter one shilling each, so that there was a clear gain of 200 per cent.

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Observations. That fruit, if not too ripe, might be preserved by the bottles in which it was packed being heated in a water bath, and then closely stopped, has been long known: but so many minute circumstances were necessary to insure success, that the addition of boiling water must be esteemed a great improvement; and we assure Mr. Saddington that we ourselves mean to put his process into execution this summer,

and have not the least doubt but that our families and guests will frequently give him their thanks in the ensuing winter.

It is expressly said that the bottles should be filled with water to within an inch of the cork: but would it not be better to fill them entirely with water? for what good purpose can be answered by leaving any bubble of air within the bottle, which renders it further necessary to turn the bottle occasionally?

It appears to us also that the water with which the bottles are filled, ought to have boiled strongly for some time, in order to disengage it as much as possible of the air it contained.

[*Lond. Retrospect.*

Account of a simple and economical Method of preparing an artificial Cheltenham Water highly impregnated with Carbonic Acid. By RICHARD GREENE, *Esq. of Cork; A. B. Trin. Col. Dub. M. D. and President of the Royal Medical Society, Edinburgh*.*

The natural Cheltenham waters have been long and deservedly celebrated for their medical uses. Their virtues are chiefly owing to the sulphates of soda and magnesia. They contain also about one-eighth in bulk of carbonic acid, which, in one gallon of the water, holds in solution nearly five grains of iron. The sulphate and carbonate of lime which exist in them seem to have no effect on their medicinal properties.

* Phil. Journ. No. 97.

The carbonic acid used in making artificial mineral waters is procured by treating any of the natural carbonates of lime, as chalk, marble, &c. with diluted sulphuric acid. The gas is then by mechanical pressure forced into the water.

The carbonate of soda and magnesia contain a large quantity of carbonic acid; the former about one-sixth, and the latter nearly half its weight. They are decomposed by sulphuric acid, and the salts of Cheltenham water are formed.

It ocurred to Dr. Greene, that if a sufficient quantity of water were present, and the gas, during its disengagement, could be confined, the effect would be similar to mechanical compression. Many experiments were made on this idea with perfect success.

The volume of carbonic acid which might be confined with safety in a half-pint bottle, was ascertained, and the quantities of the carbonates and diluted sulphuric acid required being known by calculation, the carbonates were put into the bottle with one grain of sulphate of iron, and filled with cold water, except a small space for the sulphuric acid, which being introduced, the bottle was corked and secured with a string. By a little agitation the salts soon dissolved, and the liquor became transparent. The string being untied, the cork was driven out with considerable violence, and the water scarcely differed in taste from the best soda-water.

The materials used in this experiment were nearly one-half greater than Dr. Greene recommends, which shows that the strength of the water may be much increased without bursting the bottles.

The carbonate of soda employed was the common soda of commerce, prepared from muriate of soda by the patent process; it neither deliquesced nor effloresced by exposure to the air, and gave no indication of lead, which it has been supposed to contain, when made in this way.

He prepared the carbonate of magnesia from sea-water, and could depend on its purity.

The sulphuric acid was the diluted acid of the Edinburgh college, of the spec. grav. 1.0735, and was made by adding one part of the acid of commerce, of the spec. grav. 1.852, to eight parts of water.

The following are said to be the best proportions of these substances to half a pint of water:

Carbonate of soda	40 grains troy.
——— of magnesia	20 grains ditto.
Sulphuric acid	250 grains by measure, or

a quantity equal in bulk to 250 grains of water.

In this case there is a slight deficiency of sulphuric acid, which should always be observed, for the excess of carbonic acid will dissolve any carbonate of magnesia that may remain; but an excess of sulphuric acid gives the water a disagreeable taste, and might injure the teeth, and affect the bowels, if irritable.

These quantities produce about 35 cubic inches of gas, which in a half-pint bottle is equal to the additional pressure of an atmosphere and half on the sides of the bottle, or of two atmospheres and half on the surface of the water.

The sulphuric acid employed should be diluted: in this case, if any lead be present, it will be precipitated.

The quantities here specified may not always exactly answer, as the carbonate of soda is not uniform in its composition, nor the acid always of the same strength; any excess, however, will be apparent, by an acid taste in the water after it has been exposed to the air for some time; and any considerable deficiency, by a part of the magnesia remaining undissolved. In either case the acid must be increased or diminished as indicated.

To ascertain the exact quantity of acid necessary to a given weight of the carbonates, add about an ounce of water in a wine-glass, introduce the acid gradually, and stir the mixture till they are dissolved, and no effervescence appears, taking care that the acid is not used in excess, which is known by its changing infusion of litmus or red cabbage-leaves to red. Note exactly the quantity of acid used, and make a measure containing ten grains less, which is easily done by a glass tube and a file. If this succeed well, a quantity of the same may be procured and kept in close vessels.

The bottles to be used should be previously furnished with corks, that no time may be lost after the acid is introduced; and they should remain on their sides, that no gas may escape. The magnesia should be in fine powder, to dissolve more readily. The soda should not be so, as by its slow solution the magnesia will meet the acid in a more active state. By observing these precautions, a bottle of the water may be made ready for use in less than a minute. It will keep well: shake the bottle before it is opened, as the iron precipitates in part.

The strength of the water may be easily varied by altering the quantities of the materials, observing the relative proportion of the acid to the carbonates.

For a pint bottle, the quantities are,

Carbonate of soda	80 grains.
——— of magnesia	40 ditto.
Sulphuret of iron	2 ditto.

This is a full dose for an adult, and more than should be taken for a constancy during a course of this water. The expence of preparing a pint botttle is only about one penny.

Observations. This appears to be a simple and economical method of obtaining artificial Cheltenham water, and it will probably be connected with general utility.

We have some doubts whether this idea is original, though we by no means wish to detract from the merits of Dr. Greene, to whose experiments the public will be much indebted for a ready and a cheap supply of this water.

Lond. Retrospect.

On the Chinese Method of propagating Fruit-trees by Abscission. By Dr. James Howison*.

It is stated that the Chinese do not raise fruit-trees from seeds or grafts, as is customary in Europe, but in the following

* Trans. Soc. Arts, Vol. XXV.

method: they select a tree which they wish to propagate, and fix upon a branch which will disfigure it the least by the removal; and round this, as near as conveniently may be to its junction with the trunk, they wind a rope made of straw besmeared with cowdung, until a ball is formed five or six times the diameter of the branch. This is intended as a bed into which young roots may shoot; and immediately under the ball the bark i divided down to the wood for nearly two-thirds of the circumference of the branch: a cocoa-nut shell, or small pot, is then hung over the ball with a hole in the bottom, so small that water put therein will only fall in drops, by which means the rope is constantly kept moist; a circumstance necessary for the ready admission of the young roots, and for the supply of nourishment for the branch. When the vessel has been supplied with water for three weeks, one-third of the remaining bark is cut, and the former incision carried deeper into the branch, as by this time some roots have struck into the rope, and assist in giving support. After a similar interval, the operation is again repeated, and in about two months from the commencement of the process, the roots are generally seen intersecting each other on the surface of the ball; which indicates that they are sufficiently advanced to admit of the separation of the branch from the tree; and this is best done by sawing it off at the incision, taking care that the rope, which must have become nearly rotten, is not shaken off by the operation; and then the branch is planted as a young tree.

It is conceived that a longer period would be necessary to succeed with this operation in Europe, because vegetation is so much slower here than in India, where Dr. Howison made his experiments; but he thinks that an additional month would be adequate to make up for the deficiency of climate.

The advantages of this method are stated to be, that a farther growth of three or four years is sufficient, when the branches are of any considerable size, to bring them to their full bearing state ; whereas eight or ten years would be otherwise necessary : this he saw proved from experiment at Prince of Wales's Island. The writer's experience does not allow him to speak of the success with which this method might be applied to forest trees, but he little doubts of its succeeding, and the adoption of it is recommended at all events in multiplying such plants, natives of warmer climates, whose seeds do not come to maturity in this country. He has besides frequently remarked that such branches of fruit-trees as were under the operation of abscission at the time of bearing, were more laden with fruit than the rest of the tree, which is attributed to a plethora or fulness, occasioned by the communication between the branches and the trunk being cut off by the division of the bark, and has observed that the roots from a branch under this operation were longer in shooting into the ball of straw, when the tree was in leaf, than at another time, on which account he recommends the spring as the best season for making experiments.

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Observations.—This mode of propagating trees, which prevails in many parts of eastern Asia, is deserving the notice of our horticulturists at home ; and from the prevailing inclination to pursue that branch of science, we have no doubt of being soon able to communicate experiments of this mode of propagation in our own country.

[*London Retrospect.*

Analysis of the Waters of the Dead Sea, and of the River Jordan. *By Dr.* ALEXANDER MARCET*.

The Dead Sea derives its name from its intense saltness, which prevents either animals or vegetables from living in it. Although this circumstance has been mentioned by many very ancient authors, the only chemical analysis of its waters is that by Macquer, Lavoisier, and Sage, published in the Mem. de l'Acad. des Sciences for 1778, which is very imperfect.

Mr. Gordon, of Clunie, recently brought from the Dead Sea a phial containing about 1½ oz. from a part of the lake about two miles from the mouth of the Jordan; and another somewhat larger phial of the water of the Jordan. This specimen was brought from a spot about three miles from where the river enters the Dead Sea.

The specific gravity of the water of the Dead Sea is 1.211, so that it has been found that a man may lie motionless in any attitude on its surface, without danger of sinking. It is perfectly transparent, and does not deposit any crystals in close vessels. Its taste is peculiarly bitter, saline, and pungent. It forms copious precipitations in solutions of silver. Oxalic acid throws down oxalate of lime, and this being separated, both caustic and carbonated alkalies readily throw down a magnesian precipitate. Solutions of barytes produce a cloud, which shows the presence of sulphuric acid. Succinate of ammonia does not detect any alumine in it: nor are the infusions of litmus, violet, or turmeric altered in colour. It will take up more common salt.

* Phil. Trans. 1807, part II.

From hence the water appears to contain the muriates of soda, lime, and magnesia, along with sulphate of lime. The composition, therefore, of these salts was examined.

A known measure of muriatic acid was poured upon marble, which had been previously ascertained to contain 56.1 per cent. of lime. The undissolved marble being weighed, and the solution evaporated to dryness, and ignited, the muriate of lime was found to contain 50.77 per cent. of lime, and 49.23 of acid.

A certain measure of muriatic acid sufficient to dissolve a known weight of marble, was added to calcined magnesia, and the excess of acid saturated with marble; and from thence muriate of magnesia, perfectly free from water, was found to contain 43.99 per cent. of magnesia, and 56.01 of acid.

A certain measure of muriatic acid sufficient to dissolve a known weight of marble, was treated with nitrate of silver; and from thence the luna cornea, after being melted, and heated to redness, was found to contain 80.95 per cent. of oxide of silver, and 19.05 of acid. Then nitrate of silver being precipitated by a known weight of common salt, it was found to contain 46 per cent. of acid, and 54 of soda.

In order to estimate the defects and advantages of the different methods which might be adopted for the analysis of the water, they were tried on artificial solutions, containing the three muriates found in the water, but omitting the sulphate of lime.

The first solution was evaporated to dryness, and ignited for an hour in a platina crucible, pretty closely covered. The

residuum was elixiviated with water to separate the magnesia, the lime was precipitated from the ley by carbonate of ammonia, and the common salt obtained by a new evaporation. But the muriate of magnesia was not completely decomposed by the ignition, and therefore this mode of analysis indicates less magnesia, and more lime, than was really in the solution; the estimate of common salt was tolerably accurate.

The lime of the second solution was precipitated by oxalate of ammonia, the muriate of magnesia was decomposed by heat in an open crucible; and the common salt was obtained by elixiviation and evaporation; the estimate of lime and magnesia was accurate, but the common salt was materially reduced by the heat necessary to decompose the muriate of magnesia.

In a third solution, the lime was precipitated by oxalate of ammonia, the magnesia by recent carbonate of ammonia, and the common salt obtained by evaporation and desiccation. But the magnesia was only imperfectly precipitated.

The most successful method was to separate the muriatic acid from part of the solution by nitrate of silver. From another portion the lime was precipitated by oxalate of ammonia, and the magnesia either by caustic potash or by carbonate of ammonia; in which latter case the precipitation is imperfect, and the remainder of the magnesia must be separated by exsiccation, ignition, and subsequent elixiviation. The quantity of common salt was inferred from the remaining quantity of acid, after the portions of acid belonging to the other two muriates had been calculated. This estimate may be corrected by actually obtaining the common salt from one of the portions of the solution. This method was found to be remarkably accurate.

The information thus acquired, was applied to the analysis of the water of the Dead Sea.

Twenty grains of this water, being evaporated at a heat of about 180° Fahr. left 8.2 gr. of an opake white mass, containing crystals of common salt. This mass deliquesced in the air, and being again dried at 212° Fahr. left 7.7 gr. This residuum being exposed for a few minutes to a heat about 15° higher, was reduced to 7.4 grains ; and as, on dissolving it in water, a few insoluble white particles appeared, the muriate of magnesia had begun to be decomposed.

A few drops of muriate of barytes, added to 100 gr. of the water of the Dead Sea, let fall 0.1 gr. of sulphate of barytes.

Nitrate of silver, added to 250 gr. of the water, let fall a quantity of luna cornea, which, being edulcorated and slightly ignited, weighed 163.2 gr. A little sal ammoniac was added to the supernatant solution, to separate the excess of silver, which was edulcorated and dried. The clear fluid was evaporated to about 3 oz. and a strong solution of oxalate of ammonia added warm, but not near boiling, lest it should throw down the magnesia. To ascertain the quantity of lime contained in the precipitated oxalate of lime, the oxalic acid was driven off by a low red heat, and a known quantity of muriatic acid, more than sufficient to dissolve the subcarbonate of lime thus produced, was added : the excess of acid was taken away by a piece of marble of a known weight. The quantity of lime in the selenite, according to the calculations of Chenevix, being subtracted, the oxalate of lime, thus precipitated, indicated 4.814 gr. of pure lime, equal to 9.48 gr. of muriate of lime.

The clear solution, which was about 4 oz. was concentrated to between 2 and 3 ounces, during which a white powder was deposited, which, from other experiments, was supposed to be oxalate of magnesia. To the concentrated solution was added carbonate of ammonia, with excess of pure ammonia, which rendered the mixture opake and milky, but, on the next morning, the fluid had become quite transparent, with clusters of pellucid crystals spread over the bottom of the vessel. This salt was, no doubt, an ammoniaco-magnesian carbonate, and was exposed to a gentle heat, to drive off the ammonia; the white impalpable powder, thus left, was treated, and its quantity estimated in a way similar to that which had been employed with the lime.

The supernatant solution, although its transparency was not disturbed by caustic potash, yet, on being evaporated and calcined, it left about $\frac{1}{2}$ gr. of carbonate of magnesia. This being added to the other powder, the whole indicated 11.1 gr. of pure magnesia, equal to 25.25 of muriate of magnesia.

As 12.28 gr. of muriatic acid still remained unemployed, the quantity of common salt was estimated at 26.69 grains.

From thence the water of the Dead Sea appears to contain 3.792 per cent. of muriate of lime, 10.1 of muriate of magnesia, 10.676 of common salt, and 0.054 of sulphate of lime.

Another 150 grains of the water were treated with regard to the lime and magnesia, as in the former analysis, but the acid was not separated by silver, being only calculated from the former estimation. With regard to the common salt, 13.1 gr. of it was actually separated by evaporation, the difference of which from the former statement depends on the necessity of

heating the salt to redness. In this experiment the water appears to contain 3.92 per cent. of muriate of lime, 10.246 of muriate of magnesia, 10.36 of common salt, and 0.054 of sulphate of lime; and this is probably the most accurate statement.

Although the saline contents of the water, when perfectly desiccated, are only about 25 per cent. of the water; yet if the salts be only dessicated at the temperature of 180, they will amount to 41 per cent. The common salt is scarcely at all concerned in this difference; for 100 gr. of it lost only at most half a grain by being heated to redness in a platina crucible.

The water, analysed by Macquer and Lavoisier, had probably suffered evaporation, and the proportion of salts, as found by them, is very different from that stated above; but their mode of operating did not allow of any accuracy in this respect.

The water of the river Jordan was perfectly pellucid, very soft, and had no saline taste: but chemical re-agents produced, especially when the water was concentrated by evaporation, effects analogous to those observed with the water of the Dead Sea.

Five hundred grains, evaporated at 200°, left 0.8 gr. which is only 1-300th part of the solid contents of the water of the Dead Sea. Part of this residuum was not dissolved by distilled water, and it effervesced with acids; thus showing the presence of carbonate of lime. Oxalate of ammonia threw down a precipitate weighing 0.12 gr.; ammonia and phosphoric acid threw down another precipitate, which, when the ammonia was driven off by heat, weighed 0.18 gr. The common

salt could not be separated with any accuracy, but it was probably in nearly the same proportion as in the water of the Dead Sea. Perhaps, therefore, the river Jordan may be the source of the impregnation of the Dead Sea, or the same source of impregnation may be common to both.

Observations.—From the preliminary experiments here recorded, we may learn to estimate the small reliance that can be placed upon the analyses, even of the most skilful chemists, unless they are confirmed by other analyses, made in a different manner, as we have already remarked in several instances.

[*Lond. Retrospect.*

An Obstinate Case of Gonorrhœa successfully treated with Cerussa Acetata.

To the Editors of the Medical and Surgical Spectator.

Gentlemen,

Having met with a very obstinate case of gonorrhœa, attended with a most distressing chordee, which resisted the usual antiphlogistic treatment, and anodynes, I determined to give the *cerussa acetata* a trial, as recommended in a former number of your publication. After taking three doses of the medicine, the chordee and ardor urinæ were considerably abated; he had no return of the priapisms during the night, and the following morning he experienced very little pain in evacuating his urine. By continuing this medicine, the symptoms gradually abated, and in three days he was perfectly well, without the use of an injection, or any other remedy. It is worthy of

remark, that, during the exhibition of this medicine, his appetite was improved, and he evidently gained strength. Your recommendation of the medicine having induced me to give it a trial in his case, I think it my duty to communicate to you the result.

I am, gentlemen,

Your obedient servant,

T. M. CATON.

August 26*th*, 1809.

Sulphur a Compound.

On the 27th of June, 1808, M. Curaudau read before the Imperial Institute of France a memoir, in which he proved that sulphur, hitherto regarded as a simple substance, is composed of carbon and hydrogen, and that they may be separated after having entered into the composition of a destructible body.

[*Journal des Frères Cheinaux.*

Prospectus of a periodical work, to be entitled, The American Mineralogical Journal, conducted by Archibald Bruce, M. D. Professor of Mineralogy in the University of the state of New-York.

The object of this work is to collect and record such information as may serve to elucidate the mineralogy of the United States, than which there is no part of the habitable globe which presents to the mineralogist a richer or more extensive field for investigation.

Of the utility of a publication of this kind, much might be said : it may however be sufficient to observe, that nothing has contributed more to increase and diffuse mineralogical information than the periodical works on the continent of Europe particularly those in Germany and France. At the present period, when such laudable exertions are making to improve and extend the manufactures of our own country, a knowledge of the mineral productions, on which so many of the useful arts depend, and with which nature has so liberally supplied us, becomes particularly desirable.

The mineral combinations which exist here, under forms unknown in Europe, will, we trust, procure for this journal some attention abroad.

In order therefore that the design may be carried into effect, communications from those gentlemen who may have directed their attention to this interesting branch of science, are respectfully solicited, particularly such as may relate to the geology and mineralogy of particular districts ; the history of mines, their products, methods of reduction, and improvements in metallurgy generally ; descriptions of individual specimens, their constituent principles, localities, and uses to which they may be applied in the arts; mineral waters, their situation, analysis, and use in the arts, and in the cure of diseases, &c. A part of the work will be appropriated to such useful information as may be derived from foreign and domestic journals.

The first number will be published in the beginning of January next.

Communications, post paid, to be directed to Dr. A. Bruce, New-York, or to Messrs. Collins and Perkins, the publishers.

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